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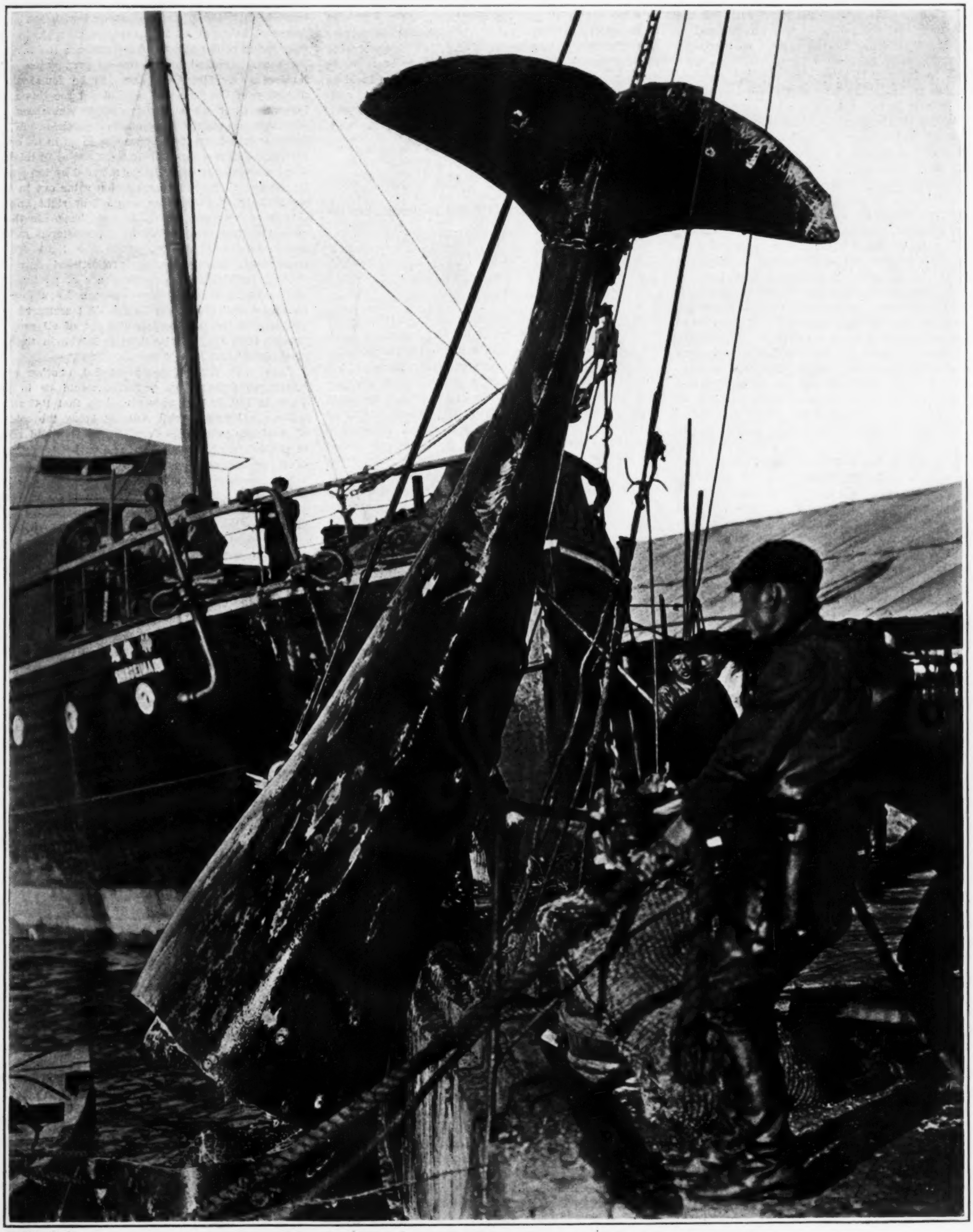

The Gilliams Service

Landing part of the catch of a Japanese whaling steamer that has just returned from a cruise

OCEAN VENISON See page 312]

Problems of Atomic Structure—II*

Differences Characteristic of Different Elements, and Mechanism of the Molecule

By Sir J. J. Thomson

[Continued from Scientific American Supplement No. 2210, Page 291, May 11, 1918]

In his first lecture the speaker had, he said, described how an estimate as to the number of electrons in an atom could be arrived at by a study of the scattering produced by them when exposed to Röntgen radiation. Provided that certain conditions were satisfied the energy in this scattered radiation was proportional to the number of electrons in the atom. He proposed now to explain more fully what these conditions were that must be fulfilled if this proportionality between the two was to hold. As he had pointed out in his previous lecture, it was not always the case that the number of the electrons in the atom was proportional to the energy in the scattered radiation, and this being so it was very necessary to see what the essential conditions were. He thought that the nature of these would come out clearly if we considered what would happen in the case of two electrons only.

Suppose two electrons A and B (Fig. 1) were exposed to Röntgen radiation coming from a distant source, O, and let the energy of the radiation scattered by them be observed at P, also at a great distance from the particles as compared with the distance A B. The energy in the scattered radiation received at P depended upon the difference in the length of the paths O B+B P and O A+A P. If from B we drew B *o* perpendicular to O A and A *p* perpendicular to B P, then, remembering that O and P were both at a very great distance from the electrons as compared with the distance A B, the two lines O A and O B might be treated as if they were parallel, as also the two lines A P and B P. Hence the difference in the total lengths of the two paths was simply A *o*—B *p*.

The difference in the phases of the pulses arriving at P from A and B respectively depended upon this difference in the path lengths. This difference had a maximum value when the scattered ray was sent back in the direction of the origin, and if the distance A B was small in comparison with the wave length, both pulses would reach P in phase, and the energy of this scattered radiation would then be proportional to the square of the number of the electrons concerned. Hence the one condition essential to success in using the

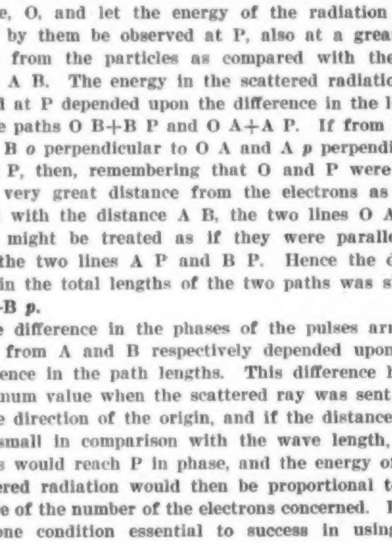

Fig. 1.

scattering of Röntgen rays for estimating the number of electrons in the atom was that the distance between the electrons must not be small compared with the wave length.

In the other extreme, when the distance A B was very large, a very small change in the angle which A B made with the incident radiation, might make a difference of several wave lengths in the paths of the two pulses arriving at P, and the relative phases of the two on reaching this point would be quite arbitrary. In that case the amplitude of the disturbance at P would be proportional to the square root of the number of the electrons, whilst the energy (which varied with the square of the amplitude) would be directly proportional to the number of the electrons concerned. Hence, when A B was very large compared with the wave length of the incident radiation, the necessary conditions for a reliable estimate were satisfied.

We had thus the solution for the two extreme cases, viz., when A B was relatively very small and when it was relatively very large. It was of interest to work out the solution for the intermediate case in which the wave length was comparable with the distance between the two electrons. In practice, of course, we had unavoidably to deal not with just two electrons, but with a number arranged in all sorts of ways, and we could accordingly only deduce the average result to be expected when the lines drawn between all possible pairs of the electrons were assumed to come out in all possible directions. As a first step to fixing this average value we might, however, consider the case of two electrons only, and determine the average result when the line connecting them made every possible angle with the direction of the incident rays. It then appeared that the energy of the light scattered to P depended upon the angle θ which the scattered ray made with the incident ray. If, in short, δ were defined by the relation

$$\delta = 2\,A\,B \sin\frac{\theta}{2},$$

then the energy scattered to P was proportional to the expression

$$2 + 2\left(\frac{\sin\frac{2\pi\delta}{\lambda}}{\frac{2\pi\delta}{\lambda}}\right)$$

This was a function which began by attaining its zenith at the very moment of its birth, being a maximum when δ was zero. As δ increased, the function did not for some time alter appreciably in value, but then senile decay rapidly set in, and it fell off abruptly and reached a minimum value, afterwards rising again to a new but lower maximum. The characteristics of the function were shown in Fig. 2, where it would be seen that the amplitudes of the variations got smaller and smaller as δ increased and they finally faded away into insignificance. It would be seen that the above expression, for the energy scattered, comprised a steady effect with a fluctuating term superimposed on it.

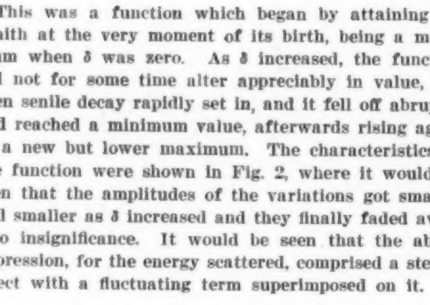

Fig. 2.

The value of δ depended upon the angle θ between the direction of the scattered light and the incident light, and if this angle were altered there should be variations in the brightness at P as the position of P was changed relatively to the incident ray (Fig. 1). Hence if the radiation were received on a screen, we should expect that there would be a series of rings alternately dark and bright surrounding a centre. This effect could be readily shown with ordinary light. Throwing a spot of bright light on to a screen and interposing in the path of the beam a plate of glass covered with lycopodium, the lecturer showed that the spot of light on the screen was then surrounded by a series of colored rings, which were due to the light scattered by the lycopodium powder and corresponded to the maxima and minima of the function given above. The diameters of the rings were, the lecturer pointed out, much smaller than of those observed in a further experiment in which the lycopodium powder was replaced by a layer of crystals of alum deposited on a glass plate. In this latter case the formation of the rings was due, the lecturer said, to refraction from the crystal faces, and was not due to scattering at all.

When, in place of two particles only, we had to deal with a number N, say, the expression for the energy comprised a number of terms of the form, where δ

$$2\left(\frac{\sin\frac{2\pi\delta}{\lambda}}{\frac{2\pi\delta}{\lambda}}\right)$$

represented in succession the distance between any one of the N particles and every one of the others. As before the effect changed with the angle θ, and was greatest when θ was zero; that was to say, when the direction of the incident ray and that of the scattered ray coincided, P (Fig. 1) being then on the left hand side of A B directly opposite to O.

The distribution of the energy in the scattered radiation was thus not symmetrical fore and aft unless A B was very large compared with the wave length. Practically there was always a large excess radiation scattered behind the object as compared with the amount scattered in front of it. Analysis showed, in short, that owing to the effects of interference the maximum effect was obtained in the line of propagation of the incident ray. Here, also, was to be found the explanation of the fact that we did not see this scattering when light passed through solids which contained, of course, an enormous number of particles. Why, in such cases, did the light appear to go on in its original direction and not flare out at right angles to this path? The reason was that the light scattered by the particles in the direction of the propagation came out in phase, whilst in that which was sent off at right angles to this there were great variations of phase. In the first case, therefore, the energy in the scattered radiation was proportional to N^2, while the light scattered transversely had an energy proportional only to N. Since in solids there were hundreds of particles in a wave length, the difference between N^2 and N was enormous, and the energy in the light scattered in the direction of the propagation was out of all proportion greater than that of the light scattered in the transverse direction.

There was, the lecturer proceeded, another case in which the pulses from particles would be in phase. Thus, in Fig. 3, if P were placed so that P C and C O made equal angles with A B (C being the mid-point of A B) the two pulses from A and B would reach P in phase. In that case the direction between the incident and scattered ray was the same as if A B were a mirror and C P the reflected ray. Hence, if there was a large number of particles in one plane, then no matter what the wave length of the incident light might be, the light scattered by the particles to a point P would have a maximum intensity when P lay in the direction in which light from O would be reflected when a mirror was substituted for the layer of scattering particles.

In all the experiments of which particulars had so far been published it had been assumed that the energy of the scattered radiation was directly proportional to

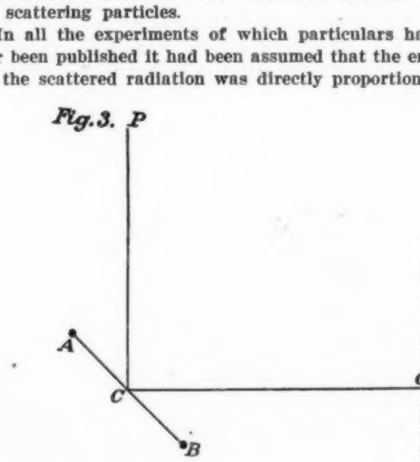

Fig. 3.

Fig. 4.

the number of electrons in the atom. The speaker was, he said, not sure that sufficient attention had been paid to the necessity of keeping the distance between the particles large in comparison with the wave lengths used and a correction of several per cent. might, he thought, be necessary. Taking the experiments as they stood, however, the observations of Barkla and Crowther showed that the number of electrons in a molecule of air was somewhere about 14, which indicated that the atom of nitrogen had 7. Other gases had also been studied, and the results indicated that the number of electrons was roughly proportional to the atomic weight, and within an error of a few per cent. could be represented by one-half the atomic weight when this was an even number, and by $\frac{A-1}{2}$ when the atomic weight A was odd.

The speaker's own impression was, however, that

*Reported in *Engineering*.

the reliability of the interpretation of these experiments had been exaggerated, and it was desirable that the experiments should be repeated, taking into account the considerations he had advanced above. He would, moreover, emphasize the fact that the possibilities of experiments on scattering had not yet been utilized to the extent of which they were capable. Experiments of this kind ought to give us, not merely data as to the number of electrons in the atom, but also some information as to their arrangement. To illustrate what he had in view, he would suppose that an atom contained four electrons and that it was desired to know how they were arranged. They might, for example, be situated at the corners of a square, or at the corners of a tetrahedron. Both arrangements were symmetrical and in both cases the electrons would be positions of equilibrium, though whether this equilibrium was stable in both cases was another matter. Neglecting this latter consideration, however, the character of the scattering effect would be different in the two cases. In the tetrahedral arrangement the length of any possible line drawn between any pair of the electrons was the same, and the expression for the energy would contain but one value of δ. If, on the other hand, the electrons were arranged at the corners of a square, the diagonals of the square were longer than the sides, and the expression for the energy of the scattered radiation would contain accordingly an additional sine term. It would seem, therefore, that if we could carry further our experiments on scattering, additional light should be thrown on the structure of the atom.

Assuming for the present, however, that the number of electrons present was equal to half the atomic weight, what was their configuration inside the atom? As to this there were two extreme views. Atoms were electrically neutral bodies. If they contained electrons, there must be something else there which carried, to use ordinary language, a positive charge of electricity, just sufficient to balance the negative charges of the electrons. The question that arose was to how this positive charge was distributed. Two extreme cases might be taken. In the first place the positive charge might be considered as concentrated in an excessively small space, just as that of the electron was. This concentrated charge occupied the centre of the atom, and the electrons were arranged around it and repelled each other whilst being attracted by the central positive charge. The other extreme view was that the positive charge was distributed uniformly throughout a sphere of large size as compared with the electron. In this case the attractive force on an electron was proportional to its distance from the centre.

The difference in the conditions necessary in the two cases for static equilibrium was great.

In both cases if an electron was to be equilibrium it must occupy some position in which its attraction for the positive charge was just balanced by the repulsive force exerted by the other electrons. When the positive electricity was distributed uniformly throughout a large sphere this equilibrium was stable; and any electron returned to its former position if displaced. Here stable equilibrium was secured simply by the mutual attractions and repulsions of the charges concerned.

In the other extreme case in which the positive electricity was assumed to be concentrated at the centre of the atom, calculation showed that positions could also be found in which the electrons were in equilibrium under the attractive and repulsive forces concerned, but it turned out that this equilibrium was unstable.

To illustrate this the lecturer floated on water a magnet representing the positive charge. This was placed with its axis vertical. Another magnet, with poles reversed, to represent a negative charge, was attached to the first, but kept from coming into actual contact with it by a distance-piece represented by the bar in Fig. 4, where *a* represents the positive charge and *b* the attached electron. It was then shown that there was a point somewhere along the line R T in which a second "electron" *c* was in equilibrium, but that if *c* were moved from this position it did not come back to it but travelled either out towards R or in towards the central charge.

In short, it was not possible to find a position of stable equilibrium for *c* when the forces of repulsion and of attraction on it both varied inversely as the square of the distance. Nevertheless, we knew that there were most important systems in the universe which were stable for all practical purposes under the action of forces of this kind. An example was provided by the planetary system. However, Mahommet's coffin would be unstable, being in a position exactly analogous to the equilibrium position for *c* in Fig. 4. Under such a system of attractions it was impossible to secure a condition of stable equilibrium unless the particle were in motion like the earth round the sun, where the attraction was balanced by centrifugal forces. He had calculated out the conditions of stable equilibrium for the electrons on an hypothesis which included that of stability due to rotation, viz., that the force on the electron was represented by the expression:

$$\frac{e\,e^1}{r^2} - \frac{c}{r^3} \quad . \qquad . \quad . \ (1)$$

where r denoted the distance between the mutually attracting masses e and e^1.

Another problem which arose was what amount of positive electricity would it be necessary to concentrate at the middle of an atom to keep the electrons in stable equilibrium under forces represented by equation (1).

If there were a great number of electrons, there would be no stability unless the positive charge was enormous, as was indicated by the following table:—

No. of Electrons	1	2	3	4	5
E/e..	0.70	$\frac{1}{4}$	1.587	3.10	4.76
No. of Electrons	6	8	10	12	14
E/e..	7.32	14.20	24.16	38.95	58.44
No. of Electrons	16	18	20		
E/e..	83.6	115.4	154.5		

When the number was less than 5, it would be seen that there was an excess of negative electricity, whilst somewhere between 5 and 6 the two charges would be equal. The table, it should be observed, had been calculated on the assumption that the electrons were arranged in a flat ring round the positive charge. This was not the most favourable condition when the number of electrons was large, but the mathematical difficulties were increased very greatly if another dimension in space had to be taken into account. He had, nevertheless, been able to calculate out some figures of equilibrium for three dimensions. The tetrahedron was very stable, and the electrons arranged at the corners of an octahedron would be stable when the charge at the centre was not greater than the sum of the charges around it. The cube was not a stable form, but became so if one face was twisted round (in its own plane) through an angle of 45 deg. The icosahedron appeared to be just about stable, but the dodecahedron with 20 electrons, one at each corner, was certainly unstable.

Referring to the table above given, which applied to the case in which the electrons all lay in a flat ring, it would be seen that with a positive charge of 150 only 20 electrons could be held in stable equilibrium round it. There was thus an excess of 130 charges left, or points situated far away from the group, the total effect of the latter would be represented by a positive charge equal to this residue, and this might support another ring of electrons under forces varying according to the general law represented by equation (1). There would still be a residue left which could support a still more distant ring. Each successive ring would consist of fewer and fewer electrons.

This was the exact converse of what was found to be the condition for equilibrium on the alternative view, in which the positive electricity was assumed to be uniformly distributed throughout a relatively large sphere; the number of electrons near the centre was then small, the greater number being in the outer rings.

Some crucial points in such investigations as these depended on the atomic volume. This was defined as the $\frac{\text{density}}{\text{atomic weight}}$ and was not quite definite unless it were assumed that the atoms were hard incompressible spheres. To illustrate the difficulty in the above definition the speaker referred to the work of Osborne Reynolds, who was the first to account for the curious fact which must have been observed by everyone who walked on wet sand, viz., that the effect of the pressure of the foot was apparently not to drive out water into the sand round the footmark, but to dry it. In short, with well-settled sand, any disturbance increased the proportion of the voids to the total volume occupied. This property had been called that of dilatency by Osborne Reynolds, and he had elaborated it into what was almost a theory of the universe.

[TO BE CONTINUED]

The Delicious Fruits of Actinidia

In the collection of vines at the northern end of the Herbaceous Garden Valley, not far from the Museum Building in the New York Botanical Garden, is a magnificent specimen of *Actinidia arguta*, which was obtained from the Department of Plant Introduction at Washington in 1898 and has been growing rapidly ever since. The past season, it fruited abundantly and the writer had an opportunity to examine and test the flavor of the fruits in various stages. Several years ago, he had a similar opportunity while visiting Dr. W. Gilman Thompson at his summer home in Stockbridge, Mass., where he found a porch covered with a large vine of this species in full fruit.

During a recent visit to Washington, Dr. Fairchild supplied an interesting publication of his on the most promising species of this genus and Mr. Bisset showed a collection of herbarium specimens and fruits. It cannot now be doubted that *Actinidia arguta* is a valuable plant, hardy in this region, which is ornamental and also yields an abundance of good, edible fruit. The fruits of the more tender species, *A. chinensis*, are considerably larger, but are covered with a densely hairy skin, so that they have to be peeled. If this skin can be changed by amelioration or hybridization, the plant should prove a valuable one for the southern United States.

The genus *Actinidia* contains over twenty species, most of them natives of eastern Asia, and about seven are in cultivation. The species are mostly climbing shrubs with handsome foilage, white or rarely pinkish flowers, and berry-like fruits with small seeds. They prefer moist, rich soil and a half-shaded or sunny exposure. They are easily propagated by seeds, cuttings, or layers. *A. kalomikta*, which is hardy in New England and eastern Canada, has rounded leaves variegated with white or pink; white flowers; and oblong fruit which is blue and sweet. The fruits are dried by Russian settlers in Siberia and put aside for winter use in bread and confectionery. *A. polygama* is hardy as far north as Massachusetts. Its leaves and twigs are very much relished by cats, so that it is necessary to protect them with wire. The two most valuable species are *A. arguta* and *A. chinensis*, which will be described a little more fully. In both of these species it is necessary either to plant both sexes or to obtain specimens with polygamous flowers if fruit is desired. The flavor of the fruit in both species varies considerably, but it is said to be always excellent.

ACTINIDIA ARGUTA

This species grows rapidly and is remarkably free from insects and diseases. The leaves are broad and shining; the flowers white and nearly an inch across; the fruits somewhat oblong, greenish-yellow, an inch or more long, with thin, smooth skin and sweet, excellent flavor, reminding one of figs. In northern Korea, the plant is known as the "Tara," or wild fig. The fruits may be eaten raw, cooked, preserved, or canned. This plant would probably have been more used in this country if so many male plants had not been distributed and if the species came into bearing sooner. The plants observed have usually required eight or nine years to produce fruit.

ACTINIDIA CHINENSIS

The leaves of this species are heart-shaped and dark green; the flowers large, creamy-white, and two inches across; the fruit, which is known in China as "Yang-Taw," is egg-shaped, one to two inches long, russet-brown, densely hairy, with green flesh and excellent flavor, resembling that of the gooseberry. When the fruits are picked and left for a few days until soft, they are very fine eating. They ordinarily need about as much sugar as strawberries do. Delicious jam, pies, and sauce can be made of them. This is the most beautiful species of the genus, having the largest leaves, flowers and fruit. Unfortunately, it is not hardy in the region about New York. It has been grown at Chico, Cal., where it flowered profusely, but all the plants proved to be males. The flowers produced were large and white, with abundant, yellow stamens, which made the male plants very attractive as ornamentals, although valueless as food producers.—W. A. Murrill in the *Journal of the New York Botanical Garden.*

Gas-Heated Isothermal Room

In the Physical Institute of the University of Königsberg a constant temperature room had been created by providing double walls with insulating layers; the room was damp in summer, dry in winter, but never comfortable. The room was heated by hot water pipes; by adding a gas heater with automatic control a pleasant temperature is maintained within ± 0.04 degrees C. The electrically controlled gas valve (described by G. Hoffmann in the *Physikalische Zeitschrift* of July 15, 1917) has the shape of a U; the limbs are wound with coils like an electromagnet, and the passage connecting the two limbs at the bottom is closed when the magnet is energized and a plate raised; a small pilot jet is always left burning. The control is effected by a spiral built up of strips of zinc and iron like a compensating pendulum; the spiral ends in a contact arm bearing against an adjustable screw. The device is inserted in a shunt of the lighting circuit; the gas is lighted when the temperature sinks below the normal. The device thus operates intermittently and keeps the temperature within the limits 18.1 degree and 18.2 degrees C.

Anomalies of the Animal World—Part X

Remarkable Forms Among the Reptilia

By Dr. R. W. Shufeldt*

THERE are four orders of animals arrayed under the reptilia or reptiles, and these have been created to include the turtles (*Chelonia*), the crocodiles (*Crocodilina*), the lizards (*Sauria*), and the snakes (*Ophidia*).

Within their own group, the crocodiles present nothing of a specially anomalous character, nor do their immediate ancestors. There are some twenty existing species of them known, and these include the Gavial of the ganges, the true Crocodiles of the African rivers, South and Central America and the Antilles; and, finally, our American Alligator.

As to tortoises, turtles and terrapins, there are various kinds of them, and ordinary forms in every kind. As a class they are easily distinguished from all the rest of the reptilia, and a great many fossil forms of them are known, some of which are of enormous size.

Turtles are marine forms, with their limbs and other parts modified for an aquatic existence. Individuals of some species may come to weigh as much as a thousand pounds.

Soft-shelled turtles are peculiar in a way, but not so with respect to their own family—the *trionychidae*. They occur only in the Mississippi valley and gulf drainage basin, and are widely known to every one interested in the life histories of animals.

Tortoises are of two kinds, those that live in the freshwater ponds and streams (terrapins), and the true land tortoises. Good example of *ordinary* tortoises are seen in the common one of Europe; in our American box tortoise, and in the painted turtle of our ponds and rivers. It is not the scheme of the present work to give the structural characters of any of these species; moreover, most intelligent people know something about them.

Unusual forms among them may be seen in the giant or elephant tortoises of the Seychelles, Aldabra, and the Galapagos islands. Formerly they occured in India, in southern Europe, and in both North and South America. Much has been written about them during the past few years, and most zoological gardens have living examples. These turtles and others have already been referred to in a previous chapter.

In Texas, Florida, and some of the other southern states we meet with a very big snapping turtle, known as Temminck's snapper. Examples of this chelonian have been kept in the tanks at the Zoological Gardens of London, at Regent's Park, where it has been studied by Mr. Saville-Kent, who writes the following in regard to it:

"It usually lies prone at the bottom of its tank, giving little or no signs of life throughout the day, but is wont to display more activity and to move about its tank at night. At times, when ready for a fresh food supply, it is observed that it will lie motionless as a stone, as usual, but with its mouth open to its widest gape. This attitude it will maintain for several hours together. The singularity of this action is that the gaping jaws displayed to view two elongate worm-like structures, which spring close at one another from the floor of the mouth just within its entrance. These worm-like appendages are continually writhing to and fro, and present, in both aspect and movement, a most remarkable resemblance to actual living worms. With this naturally provided decoy for fish, there can be no need for the snapper to exhaust its energies in the strenuous pursuit of its quarry. To make the delusion complete, the head, neck and chin of this snapper are decorated with small lobular or leaf-like membranous appendages resembling sponges or aquatic vegetation. The solid, grey-brown, triangular head of the animal itself might easily be mistaken for a piece of rock, and thus decorated with seemingly natural growths the unwary fish come browsing along it, rush upon the wriggling worms at the entrance of the cavernous chamber, and are lost."

Temminck's snapper has been observed by me in the lagoons in various parts of the South; but its peculiar habits, so well described by Mr. Saville-Kent above, can only be thoroughly studied when the animal is placed under proper conditions in captivity.

*(Dr. Shufeldt served his country in the Civil War, and in the Indian Wars, but has been on the retired list for a number of years. Some time ago, at his own request, he was restored to the active list, with the rank of Major in the U. S. regular army, and has been placed in charge of the Museum of the surgeon general's office, an important appointment that is a gratifying recognition of his scientific attainments.)

Fig. 1. Flying Dragon

There is a remarkable tortoise in northern Brazil, having some affinity with Temminck's snapper, known as the Matamata tortoise. The *foliaceous decoy* of the head and neck is more luxuriant than in our species, just described, and it will be interesting to ascertain whether it likewise possesses the worm-decoys growing within its mouth.

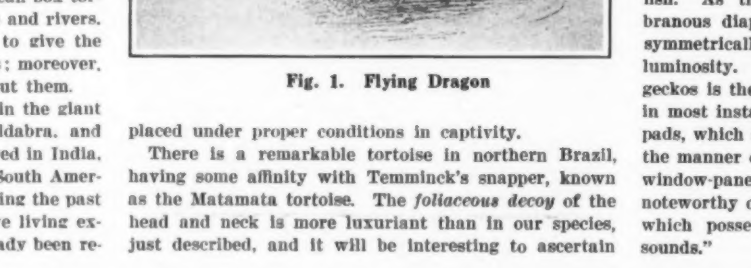

Fig. 2. The Glass Snake (O. apus) from life by the author after W. P. Dando P. Z. S.

The long-necked aquatic tortoises of Australasia and South America are remarkable for their *snake-like* heads and necks which, when drawn into their shells, are folded along the anterior margins of the body, and not twice flexed in the vertical plane as in our common box tortoise.

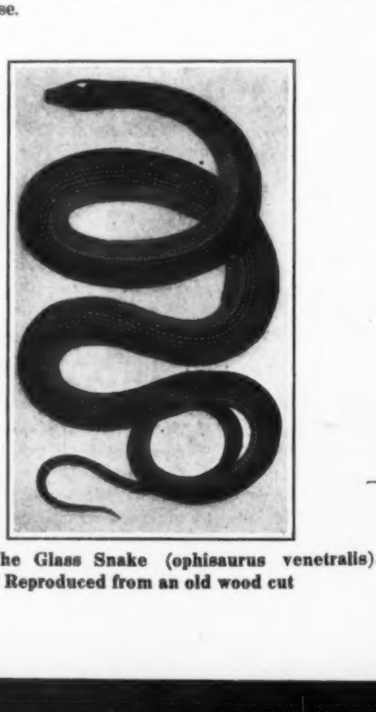

Fig. 3. The Glass Snake (ophisaurus venetralis). Reproduced from an old wood cut

There are upwards of 2,000 different species of lizards known in the world, to say nothing about subspecies. Early in geologic time, they arose from a stock common to them and to birds; therefore, as one would naturally expect, we have found many fossil lizards, and these, the older they are with respect to time, the more they depart in the matter of their skeletons from existing species. Lizards are, likewise, structurally more or less linked, through certain forms, to the ophidians; in fact, as will be shown further on, some limbless ones have been called snakes by the layman.

Professor Huxley relegated the reptilia and birds to one extensive group, the *Sauropsida*.

The common green and ocellated lizards of England and the continent are excellent examples of ordinary lizards, as is our abundant little fence "swift" (*Scloporus*) and others. Such forms present no especial anomalies as are, for example, possessed by the Geckos which, as Mr. Saville-Kent says: "present several somewhat anomalous features and characteristics. In the first place, in contradistinction to the majority of lizard forms, they are for the most part nocturnal in their habits, and have their eyes specially modified to meet them. Geckos, as the exception to the ordinary lizards, possess no eyelids, and the pupil of the eye, as seen in broad daylight, is mostly represented by a narrow, vertical slit, like that of a cat or a nocturnal dogfish. As the night approaches, however, the membranous diaphragm is retracted, displaying to view a symmetrically orbicular pupil of abnormal size and luminosity. Another prominent characteristic of the geckos is the peculiar modification of their feet, which in most instances are furnished with adhesive disks or pads, which enable these lizards to run with ease, after the manner of flies, on the smooth surface of a wall or window-pane, or even along the ceiling. It is further noteworthy of the geckos that they are the only lizards which possess the power of emitting distinct vocal sounds."

These curious representatives of the saurian group are found in the Indian and Australasian regions; they range in size from a few inches to a foot.

It is a well-known fact that the tails of lizards easily fracture off, and, in the course of time, a new tail grows out to replace the one lost. Geckos form no exceptions to this rule, while the anomaly presented in respect to it is that, instead of one tail, two or three will sprout in their case, when the original is broken off.[1]

At least one lizard enjoys, to a certain degree, the power of sailing through the air, as in the cases of flying squirrels flying frogs of Borneo, and some other animals. These lizards are called flying dragons (see Fig. 1). and they are all small-sized forms found in the Indo-Malayan region. They possess elongated ribs in mid-series, some five to seven pairs of them; these ribs support, on either side of the body, a semitransparent membrane, it being stretched over them both dorsally and ventrally, united at the free margins, and continuous with the general integument of the body. It, or rather, these "wings," close up like a fan when not in use, and fall to the sides of the animal; but when spread form a parachute of marked effectiveness, as by its use this lizard can leap from the limb of a tree and sail to another one at certain distances as well as a Phalanger or a flying Lemur.[2]

Sometimes these "wings" in certain species of the flying dragons are beautifully marked, resembling the wing-markings of some butterflies.

From lizards possessing the power to sail through the air, and others that can walk along the ceiling, we may pass to species shorn of any and every faculty of the kind. Examples of these latter are seen in the *limbless* lizards of this country and the continent, generally called glass-snakes, Figs. 2 and 3, and in the south of Europe likewise Scheltopusiks. These rep-

[1]Shufeldt, R. W. Observations on the Habits of the American Chameleon (*Anolis principalis*). Amer. Nat. vol. xvii, No. 9, Phila., Sept., 1883, pp. 919-926. Illustrated. Gives examples of the second growth of tails in lizards, following the loss of that appendage through accident or violence.

[2]Mivart, St. George. "The Common Frog." P. 33 for a figure of the skeleton of a Flying Dragon.

tiles possess no *external* limbs, though upon dissection they display rudimentary ones with their arches.[5]

What is most anomalous about them is, however, that they part with their tails with far greater celerity than do other saurians. On the slightest provocation, not only will the tail part company with the body—a fracture that always takes place posterior to the vent or cloacal opening—but very frequently the tail will, in falling, break up into several other pieces; hence the name of "glass-snake." But, as has elsewhere been shown by me, these pieces never unite again and form their original attachment to the end of the body, as we hear the story from those who can believe such myths and silly fables.

These legless lizards possess movable eyelids, and conspicuous external openings to the ears, which no snake known to me ever does, although some lizards are likewise devoid of eyelids.

There are some lizards that can, and when need be habitually do, run with great rapidity on their hind legs, holding up the fore pair in front of them at the time. The frilled lizard of Queensland, Australia, practices this bipedal locomotion, as does the big lizard known in the same region as Leseurs' water-lizard, a species that grows to be three or four feet long. No more anomalous lizard is known to me than the aforesaid frilled lizard of Western Australia. Around its neck there is developed a great, loose frill, here shown in Figures 4 and 5. This frill the owner can raise or let fall at its own volition (Fig. 5). When in a fighting mood, or the creature desires to alarm an enemy, it opens its mouth to the fullest extent, and raises its frill or collar to a flaring appendage, standing out at right angles to its neck. This then most dangerous appearing saurian will confront a big dog, and the latter will often back away rather than attack it. The frill is highly colored, moreover, generally scarlet and orange, which in no way detracts from its use as a structure to deter the advance of its owner's enemies. An adult of this species will average some two feet in length, and possess a frill or collar nine inches across at its widest part.

When running at full speed on its hind legs, and holding its fore limbs out much as one of us would do, it presents a curious resemblance to a human being on the "dead run," as may be appreciated by turning to Figures 6 and 7.

Saville-Kent, who was the first naturalist to photograph these remarkable lizards, and probably the first to bring them alive to England, assures us that this erect attitude is only assumed by the frilled lizard in going about, when it has a considerable distance to travel on comparatively level ground. Ordinarily it progresses on all fours after the fashion of other species of the group.

Other curious Australian forms are the mountain devil, and the Jew or bearded lizard, the latter having a peculiar skin-appendage on its chin that it erects when angry, frightened or excited.

Our American "horned-toads" or spiny phrynosomas, numerous species of which are found throughout the West and Mexico, are sufficiently well known to not require special description here. That they have a habit of jetting blood—or a fluid closely resembling it—from the eyes, under some circumstances, seems now to have been fully established. No other lizard seems to have this habit.

The common tuberculated iguana is a lizard that may grow to be six feet long. It is a handsome creature and can swim well; while the big sea-lizard of the Galapagos, discovered by Darwin, can be held down with a weight under water for over an hour without apparently inconveniencing it in the least.

Most people knowing anything about lizards are now more or less familiar with our gila monster or heloderma, of which there are two species known, the Mexican form ranging only through that country.

Many believe the bite of this lizard to be highly poisonous, an opinion not concurred in by the present writer who, in 1884, was most severely bitten by a large heloderma under conditions that, had the reptile been capable of inflicting a bite of the nature referred to, death would surely have resulted. As it happened, scarcely any inconvenience was experienced from the wound. However, it is stated that some experimenters have had helodermas bite guinea pigs and chickens, and death result from such wounds; while others have utterly failed in similar experiments,

[5]Shufeldt, R. W. Remarks upon the Osteology of Opheosaurus ventralis. Proc. U. S. National Museum, 1881, pp. 392-400, figures 1-9. An account of the skeleton in a glass-snake, which was read before the Biological Society of Washington, Dec. 23, 1881.

Fig. 4. Frilled Lizard at bay with frills expanded, after a photo by W. Saville-Kent

Fig. 5. Frilled Lizard with frill folded. After a photo by W. Saville-Kent

Fig. 6. Frilled Lizard from photographs made by W. Saville-Kent

Fig. 7. Showing how the Frilled Lizard runs on its hind legs

as did some of those of Doctor Yarrow at the Smithsonian Institution.

There is a small species of lizard found in the south of England that is viviparous, which is anomalous for the reason that most all lizards lay eggs from which their young are brought forth.

Among other remarkable and more or less anomalous lizards, there may be mentioned the monitors, the girdled lizards of South Africa, the Australian stump-tailed lizard, the tail of which latter very closely resembles the animal's head; in fact, Dampier, over three centuries ago, described the caudal end of this species as a head!

Finally, there are the numerous species of true Chameleons, the life histories of which would furnish material for a volume. They present no end of anomalies in habits and appearance, to say not a word with respect to structure and physiology.

In short, the list of reptiles of this extensive Saurian group that offer us strange anomalies for study is a long one; so that the species referred to in the present chapter give but a hint as to what might be written upon such a theme.

Ancestral forms of early Saurian types, now extinct for years to be reckoned by the million, were not only noted for their extraordinary appearance in most instances, but for their gigantic proportions. Much magnificent scientific work has been accomplished within recent years with respect to the restoration of these extraordinary monsters from their fossil remains.

Several of our museums contain the results of these, especially the American Museum of Natural History in New York City, and the National Museum at Washington, D. C., where Mr. Gilmore and his corps of assistants have placed on exhibition some truly marvelous restorations of these ancient lizards.

Among them we find the herbivorous and carnivorous Dinosaurs, the *Triceratops*, *Ceratosarus*, the extraordinary *Stegosaurus*—one of the armored Dinosaurs of the Jurassic, and not a few others.

[6]Shufeldt, R. W. Contributions to the study of *Heloderma suspectum*. Proc. Zoöl. Soc. London. Pt. ii, Lond. 1890 (April 1), pp. 148-244, Plates xvi-xviii. This brochure presents a full account of its anatomy, and a complete bibliography. This writer has also published some twenty other articles on Helodermas, and numerous photographs from life of this lizard.

Iodine and Iodine-Thiourea as Subtractive Reducers for Photographic Negatives and Positives

Iodine can be used as a photographic reducer in solution in potassium cyanide, in potassium iodide, or in alcohol and also in combination with thiourea. The solution in cyanide is not satisfactory in use because of the continued action after removal of the image from the reducing bath; treatment with "hypo" stops the action of the iodine but nor that of cyanide, which is still considerable as cyanide itself has a definite reducing action. The other methods of using iodine are free from this defect, "hypo" having an immediate arresting effect on their action; on this account very thorough washing is necessary before treatment with an iodine reducer. Very dilute solutions are recommended: For plates (a) 1 to 4 c.c. ot iodine-potassium iodide solution (1:2:200) in 100 c.c. of water; (b) 2—8 c.c. of the iodine-potassium iodide to 100 c.c. ot 4 per cent thiourea solution; for paper, solutions about half the strength ot those for plates or a solution of from 4 to 16 c.c. of iodine tincture (1:100 c.c. of 95 per cemt alcohol) to 100 c.c. of 50 per cent alcohol. The time necessary tor reduction varies from 1 min. to about 6 mins. with the first two baths and up to as long as 10 mins. with the alcohol solution. The potassium iodide and alcohol solutions give a yellowing of the image by the formation of silver iodide but it is quite easy to judge the amount of reduction. A final bath of "hypo" is necessary to dissolve the iodide and to arrest the action of the reducer, for the latter reason also after the thiourea bath. The strength of the thiourea solution must not be appreciably higher than 4 per cent, as although the authors were unable to confirm previous statements that thiourea has itself a reducing action, in stronger solution its destructive action on the gelatin film is quite marked. The reducing action of iodine was compared with that of other reducers; it resembles cyanide and Farmer's ferricyanide-hypo reducer in acting evenly over the whole image, in contrast with permanganate which acts proportionally to the depth of the image and copper chloride and ammonium persulphate which act more strongly on the shadows. The solution in potassium iodide but not the alcohol or thiourea solution gives the usual blue color with papers containing starch; it disappears, however, immediately in thr subsequent "hypo" bath.—Note from *Jour. Sec. Chem. Ind.* on an article by S. Becher and M. Winterstein in *Z. wiss. Phot.*

The Salvage of U-Boat Victims*—II

Methods by Which Many May Be Recovered After the War

By A. Russell Bond, Managing Editor of the Scientific American

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While this method of salvage seems like a very logical one for work in the open sea, one is apt to forget how large the pontoons must be to lift a vessel of any appreciable size. Not only must they support their own dead weight together with that of the sunken vessel, but some allowance must usually be made for dragging the wreck out of the clutches of a sandy or muddy bottom. Imagine the work of building pontoons large enough to raise the "Lusitania." They would have to have a combined displacement greater than that of the vessel itself and they would have to be so large that they would be very unwieldy things to handle in a seaway. It is for this reason that submarine pontoons are not often used to take the entire weight of the vessel. So far they have been employed mainly to assist in raising the sunken ship. But after the war we may expect to see a far more extensive use of them.

The main problem, however, is to get hold of the wreck. Ships are not built so that they can be picked up by the ends. Such treatment would be liable to break their backs in the middle. Were they built more like a bridge truss, the salvor's difficulties would be materially lessened. It would be a much simpler matter to raise a vessel with pontoons were it so constructed that the chains of the pontoons could be attached to each end of the hull. But because a ship is built to be supported by the water uniformly throughout its length, the salvor must use a large number of chains, properly spaced along the hull, so as to distribute the load uniformly and see that too much does not fall on this or that pontoon.

All salving operations require the services of divers, so that if there were no other limiting factor, the depth to which a diver may penetrate and perform his duties sets the mark beyond which salvage as now conducted is impossible.

A common diver's suit does not protect the diver from hydraulic pressure. Only a flexible suit and a thin layer of air separates him from the surrounding water. This air must necessarily be of the same pressure as the surrounding water. The air that is pumped down to the diver serves not only to supply his lungs, but by entering his blood transmits its pressure to every part of his anatomy. As long as the external pressure is equalized by a corresponding internal pressure, the diver experiences no serious discomfort. In fact, when the pressure is not excessively high he finds it rather exhilarating to work under such conditions, for with every breath he takes in an abnormal amount of oxygen. When he returns to the surface he realizes that he has been working under forced draught. He is very much exhausted and he is very hungry. It takes a comparatively short time to build up the high internal pressure, which the diver must have in order to withstand the pressure of the water outside, but it is the decompression when he returns to the surface that is attended with great discomfort and positive danger. If the decompression is not properly effected the diver will suffer agonies and even death from the so-called "Caisson Disease."

We know now a great deal more about the effect of compressed air on the human system, and because of this knowledge divers have recently descended to depths undreamed of a few years ago. When a diver breathes compressed air the oxygen is largely consumed and exhaled from the lungs in the form of carbon-dioxide, but much of the nitrogen is dissolved in the blood and does not escape. However, like a bottle of soda water, the blood shows no sign of the presence of the gas as long as the pressure is maintained. But on removing the pressure suddenly, the blood turns into a froth of nitrogen bubbles, just as the soda water froths when the stopper of the bottle is removed. This froth interrupts the circulation. The release of pressure is felt first in the arteries and large veins. It takes some time to reach all the tiny ramifications of the circulatory system, and serious differences to pressure are apt to occur that often result in the rupture of blood vessels. The griping pains that accompany the "Caisson Disease" are excruciating. The only cure is to restore the blood to its original pressure by placing the patient in a hospital lock or boiler-like affair where compressed air may be admitted, and then to decompress the air very slowly.

It is possible to relieve the pressure in a bottle of soda water so gradually that the gas will pass off without the formation of visible bubbles, and that is what is sought in decompressing a diver. After careful research it has been found that the pressure may be cut down very quickly to half or even less of the original amount, but then the diver must wait for the decompression to extend to the innermost recesses of his being and to the all tiny capillaries of his venous system.

In the salvage of the "F-4," a diver went down three hundred and six feet, and remained on the bottom half an hour. The pressure upon him was 135 pounds per square inch, or about one hundred and forty-five tons on the surface of his entire body. Some idea of this pressure may be had when we consider that the tallest building in the world does not bear on its foundations with a greater weight than two hundred and fifteen pounds to the square inch.

It took this diver a very short time to go down. On coming up he proceeded comparatively rapidly until he reached a depth of one hundred feet. There he found the bottom rung of a rope ladder. On it he was obliged to rest for several minutes before proceeding to the next rung. The rungs of this ladder were ten feet apart, and on each rung the diver had to rest a certain length of time, according to a schedule that had been carefully worked out. At the top rung, for instance only ten feet from the surface, he was obliged to wait forty minutes. In all, it took him an hour and forty-five minutes to come up to the surface. The decompression was complete and he suffered no symptoms of the "Caisson Disease." But he was so exhausted from his efforts that he was unfit for work for several days. Yet the operations that he performed at the depth of three hundred feet would not have taken more than a few minutes on the surface.

Attempts have been made to construct a suit that will not yield to the pressure of the sea, so that the diver will not be subjected to the weight of the water about him, but can breathe air at ordinary atmospheric pressure. Curious armor of steel has been devised, with articulated arms and legs, in which the diver is completely encased. With the ordinary rubber suit, the diver usually has his hands bare, because he is almost as dependent upon the sense of touch as a blind man. But where the pressure mounts up to such a high degree that a steel suit must be used no part of the body may be exposed. If a bare hand were extended out of the protecting armor it would immediately be mashed into a pulp and forced back through the opening in the arms of the suit. The best that can be done then is to furnish the arms of the suit with hooks or tongs or other mechanical substitutes for hands which will enable the diver to make fast to the wreck or various parts of it. But if a diver feels helpless in the bag of a suit now commonly worn, what would he do when encased in a steel boiler; for that is practically what the armored suit is! A common mistake that inventors of armor suits have made is to fail to consider the effects of the enormous hydraulic pressure on the joints of the suit. In order to make them perfectly tight, packings must be employed, and these are liable to be so jammed by the hydraulic pressure that it is well nigh impossible to articulate the limbs. Again the construction of the suit should be such that when a limb is flexed it must not displace any more water than when in an extended position, and vice versa. A diver may find that he cannot bend his arm, because to do so he would expand the cubical content of his armor by a few cubic inches, and to make room for this increment of volume it would be necessary for him to lift several hundred pounds of water. The hydraulic pressure will reduce the steel suit to its smallest possible dimensions which may result either in doubling up the members or extending them rigidly.

Other schemes have been devised to relieve the diver of abnormally high air pressure. One plan is to construct a large spherical working chamber strong enough to withstand any hydraulic pressure that might be encountered. This working chamber is to be equipped with heavy glass ports through which the workers can observe their surroundings in the light of an electric searchlight controlled from within the chamber. The sphere is to be lowered to the wreck from a barge, with which it will be in telephonic communication and from which it will be supplied with electric current to operate various electrically-driven mechanisms. By means of electromagnets this sphere may be made fast to the steel hull of the vessel and thereupon an electric drill is operated to bore a hole in the ship and insert the hook of a hoisting chain. This done, the sphere would be moved to another position, as directed by telephone, and another chain made fast. After enough of these chains have been attached to the wreck their upper ends would be secured to pontoons and the wreck would be lifted with the aid of the tide, or by mechanical means as already explained. But this scheme does not overcome the chief obstacle to salvage in the open sea—and that is the danger of storms.

However, there has been another recent development which may have a very important bearing on this problem of deep sea salvage work. It has often been noted that a submerged reef, twenty or thirty feet below the surface, may act as a breakwater to stop the storming waves. An inventor who has studied this phenomenon arrived at the theory that the reefs set up eddies in the water which broke up the rhythm of the waves and converted them into a smother of foam just above the reef. Thereupon he conceived the idea of performing the same work by means of compressed air. He laid a pipe on the sea bottom forty or fifty feet below the surface and pumped air through it. Just as he had expected, the line of air bubbles produced exactly the same effect as the submerged reef. They set up vertical currents of water which broke up the waves as soon as they struck this barrier of air. The "pneumatic breakwater," as it is called, has been tried out on an exposed part of the California coast, to protect a long pier used by an oil company. It has proved so satisfactory that the same company is now constructing another breakwater about another pier nearby. There is no reason why a breakwater of this sort should not be made about a wreck to protect the workers from storms. Where the water is very deep, it would not be necessary to lay the compressed air pipe on the bottom, but it could be carried by buoys at a convenient depth.

Altogether, the prospects are very bright for the recovery of many ships or at least of a large part of their cargo even though they may lie in water three hundred feet deep. It behooves us to devise means for conducting such deep water salvage, for if we do not do this work we may be sure it will be undertaken by the enemy. The Germans have not been carrying on their submarine warfare without a thought for the future. Last May, when the U-boats were making frightful inroads into Allied shipping, there was an item in a German newspaper calling attention to the rich treasure that was being piled up in the sea, and stating that the German wrecking companies were planning to recover this treasure on a large scale after the war. It was not a matter of chance then that most of the U-boat victims have been sunk at the very tantalizing depth of three hundred feet. German salvage companies are making unusual preparations for deep-water diving operations. Some of the ingenious German inventions are pictured in a catalogue of a very prominent diving equipment company that has been published since submarines began to claim merchant vessels as their victims. Their preparations for deep-sea work is shown in a submarine working chamber, which may be permanently located on the bottom of the sea close to the point where the salvage operations are to take place. This chamber consists of a large steel box which is supplied with air from the surface and in which the divers may make themselves comfortable when they need a rest after arduous work. Entrance to the chamber is effected through a trap door in the floor, the pressure of the air inside prevents the water from rising into the chamber and flooding it. From this submarine base the divers may go out to the wreck, either equipped with the ordinary air-tube helmets or with self-regenerating apparatus which makes them independent of an air supply for a considerable period of time. When the diver has worked for an hour or two, or when he is tired, he may return to this chamber, remove his helmet, eat a hearty meal, take a nap if he

*A lecture delivered before The American Institute of the City of New York, Polytechnic Section.

needs it, and then return to the salvage work without going through the exhausting operation of decompressing.

The work of the diver usually consists of far more than merely passing lines under a sunken hull. It is constantly necessary for him to cut away obstructing parts. He must sometimes use blasting powder. Pneumatic cutting tools frequently come into play, but the Germans have lately devised an oxy-hydrogen torch for under-water use, with which the diver can cut metal by burning through it. This is accomplished by using a cup-shaped nozzle through which a blast of air is projected under such pressure that it blows away the water over the part to be cut. The oxygen and hydrogen jets are then ignited electrically, and the work of cutting the metal proceeds in the hole in the water made by the air blast.

The diver's sled is a curious German invention. It is a sled provided with vertical and horizontal rudders which is towed by means of a motor boat at the surface. The diver seated on the sled, and provided with a self-contained diving suit, can direct the motor boat by telephone and steer his sled up and down and wherever he chooses. Thus, without any physical exertion, he is enabled to explore the bottom of the sea and hunt for wrecks.

Clearly, Germany is making great preparations for peace and intends to be so far ahead of other people in salvage methods that after the war most of the treasure in the sea will fall to her lot. She feels confident of winning on land and then she fondly believes that the sea will be hers as well. But we are not asleep. We are doing some inventing ourselves; and in a contest of ingenuity as well as in that of powder and steel the Yankee when thoroughly aroused may be counted on to come out ahead.

Waste Land and Agriculture*

LECTURING on the above subject, Dr. E. J. Russell, F. R. S., said that there were in Great Britain some 56,000,000 acres of land of which we cultivated only something like 32,000,000, leaving a balance of 24,000,000 acres which were not cultivated. A little less than half of that—something over 11,000,000 acres—was occupied by towns, cities, railways, and roads and in the nature of things could not be cultivated, but there were some 13,000,000 acres that were not so occupied, that were not fully used agriculturally and might be called either waste land or undeveloped land, whichever expression was preferred. In a country like England, it was presumed that any land that was not cultivated could not be cultivated, or, at any rate, not profitably. It by no means followed that these 13,000,000 were all hopeless, and in the march of science and the progress that had been made in general agricultural investigation it had often been found possible to deal with land that otherwise could not have been touched, though the process still remained expensive.

The problem of dealing with waste land had always attracted attention from thoughtful people interested in the land, but nothing much had been done in recent years in this country. A great deal had been done in the early days of the last century, but of late there had been so little progress that the extent of waste land had rather increased than diminished. In order that land might be cultivated it was obviously essential that the conditions should be favorable to the growth of crops. Limits might be set either by the climate or the soil; so that generally speaking there were two sets of conditions which had to be taken into account in deciding whether land could be cultivated or not. Unfortunately the climatic conditions seemed to be outside our control altogether. It seemed impossible to alter the amount of sunshine or rainfall. The only way was to attack the other side of the problem and alter the plant by proper breeding methods, and produce something which was rather better adapted to the climate than the plants we grew. This was being done to a great extent in several parts of the world, and it was proving effective in circumventing the effect of climate. It was also necessary to determine what soil conditions were needed by the crop. It must have nutrient materials, air, water, and suitable temperature, and there must be an absence of injurious conditions. Any of these that was lacking constituted a limiting factor which prevented the plant from making growth. In any attempt at reclamation the first thing to do was to find out the limiting factor. Taking waste and uncultivated land generally, it would be found that there were some four or five limiting factors. There might be an insufficiency of plant nutrients, an excess of water or a deficiency of water, a deficiency of temperature or shallowness or insufficient depth of soil.

In the fen district it was frequently necessary that water should be kept out of the land, and this was accomplished by draining on broad engineering lines. A second type of wet land, and one that was far more common, was the case where the land owed its wetness to the fact that the water could not escape. All over the country there were considerable areas of such land, and the proper way to deal with them was to find a way out for the water, which in practice was not always easy. The ancient method was to dig a ditch or open drain at certain intervals so that the proper area was drained. The disadvantage of this method was that the walls of the ditch were apt to fall in. In Roman times it was customary to fill the bottom up with stones and put soil on the top so that the water would find its way down and so get away. Draining, though an old art, was forgotten for a long time: it was not mentioned in British books in the great revival of the 16th century. Gervase Markham, for instance, wrote books on almost every branch of farming, but never one on draining. By the middle of the 17th century, however, it was common, and it was dealt with in Blith's "Improver Improved." But it was not until Puritan times that men began to realize that it was necessary to take levels in order to lay ditches properly.

The next great improvement was to try prevention instead of cure. In the 18th century a young man named Elkington inherited a very wet farm from his father, who was a Warwickshire farmer; the land lay on the side of a hill, and much of it was very wet and boggy. Elkington described how his sheep died in hundreds and his crops were ruined. Drainage was of no use, and in desperation one day he took a crowbar from a laborer who was digging a trench and drove it with great force into the ground. To his astonishment out ran a stream of water. He then realized that he had struck a spring, and it occurred to him that if he carried his drain right down to the level of the spring so that the water was carried away at the source he would have no further trouble. He therefore sank a deep drain, got down below the spring, and made a broad ditch which carried the water away. He applied the principle to the rest of his farm, and it became beautifully dry so that he was able to cultivate it properly. That led to a considerable amount of draining in the Midlands and other parts of England, and Parliament voted Elkington £1,000 for his discovery. From about 1800 to 1850 a very great amount of deep drainage was done, but the very success of Elkington's method proved its undoing. The underlying principles were not really understood, and consequently it was used in all sorts of cases where it was wholly unsuitable, so that it fell into disrepute. Drains were made four or five feet deep with the idea of tapping the sub-soil water before it came to the surface, but often there was no sub-soil water to tap. In some cases the method answered extraordinarily well, in others it failed, and hundreds of thousands of pounds were wasted. It was not realized until many years afterwards that water may come from two sources—some from below and a great deal from rain; so somewhere about 1860 shallow draining was adopted so that as the rain came in it could be got away. Eventually it was realized that both principles contained a great deal of truth.

The next development was pipe drainage, which was followed by the great Government undertakings of the last century. There was no better illustration to be found for the necessity of basing great schemes like these on a foundation of scientific principles. The work was carried out empirically, and a great deal of it wrongly. Some of it proved extremely ineffective and also very costly, and in modern days most people had not been able to carry it out; consequently drainage fell for some time into disuse, and probably during the last 20 years there had been very little done systematically. A good deal of our uncultivated land owed its sterility to the fact that the drainage had not been attended to.

A method of drainage which had come into considerable use was by means of the mole plough, which imitated the action of the mole. A steel cylinder with a pointed end was forced through the soil from 9 to 18 inches below the surface, and it was found that this tunnel was remarkably stable; it would last for some years—there were many cases on record where it had lasted from 10 to 15 years without tumbling it. The system had the obvious advantage of cheapness, the cost being about £1 per acre, especially in the Midlands. This promised to be one of the most effective methods where the conditions were suitable—the surface even and the soil free from stones. But, like others, the mole system was not entirely new, there having been mole ploughs in 1800. If, however, any drainage scheme was to be a success the whole of the area must be treated uniformly.

Another great trouble in connection with uncultivated land was its dryness. A good deal of our waste land owed its uncultivated condition to the fact that it was too dry, the water content being insufficient to allow of growth. The old way of dealing with land of this sort was to have recourse to irrigation, which was done in a great many countries. Direct addition of water is one of the very oldest agricultural practices. When history begins there was already a network of irrigation canals between the Tigris and the Euphrates, and the irrigated land was made to bear two or even three crops of wheat a year, yielding a two hundred to three hundredfold return. In England irrigation presented many difficulties, and consequently it was not usually carried out. The difficulty was got over by making full use of the water already present in the soil or which fell upon the soil. Most of our land received at least 20 inches of rainfall, and if that was husbanded there was sufficient for all ordinary crops.

If the overcoming of wetness was the great triumph of our grandfathers, we could reasonably claim for our generation that it had gone a long way towards overcoming the difficulty of dryness. Considerable parts of Britain were affected in this way, especially the sands and gravels of the eastern and southeastern counties. The old English farmers hated dry soils; they cultivated the loams and clays but left the dry sands as wastes. Nowadays one sought to increase the stock of water in the soil either by adding more water, or by diminishing the losses.

There were two ways of husbanding the water supply. One was to increase the capacity of the soil for absorbing water. If either clay or organic matter were added to these dry soils their power of holding water was increased. One of the great advantages of farmyard manure and other organic matter was that it enabled the soil to do this. There were two or three ways in which organic matter might be got into the soil. One was to grow the crop and dig it in. Another was to grow the crop and allow it to be eaten by the sheep or cattle on the land, so that their excrement would fertilize the soil. That had many advantages, but also the disadvantages that it required considerable capital, and also a knowledge of the management of sheep and cattle, which an arable farmer did not always possess.

There was great difficulty in reclaiming waste lands where there was an insufficient depth of soil. Unfortunately we did not know how to deal with this problem properly. Land of this sort could not be cultivated with our present methods. The only way in which they could be utilized on our present knowledge was to graze them roughly with sheep, and that was done to some extent, but it was exceedingly difficult to know how to get any further. Such land might serve other purposes. Gravel and coarse sand both made good residential sites, golf courses, etc., but our present methods were of little use for the purpose of converting them into agricultural land. There were cases, however, where soil would accumulate through washing down into the hollows or depressions. It was only when the soil became 5 or 6 inches deep that cultivation became possible, otherwise all that could be done was to improve the grass on the higher parts. One case, however, was susceptible to treatment. If the rock was only a thin layer and was underlain by good material there were possibilities of improvement. In its original state the soil was so thin that cropping was impossible; it was, therefore, left waste. But underneath the rock lay mineral matter that had in it the making of a good loam. When the rock was removed, and the upper and lower layers joined, a useful soil resulted. Some of the land now was so good that it let at £2 per acre; it had become valuable agricultural land, whilst formerly it was waste. The removal of rock was, of course, a serious business. This particular reclamation was brought about between 1814 and 1818, when wheat was higher in price even than it is now, and labor was vastly cheaper. Steam tackle might prove simpler, but under modern conditions it would probably be easier to blow the rock out with dynamite —a mode of "culture" which had already begun to appear before the war.

*From a paper read before the Nottingham Section of the Society of Chemical Industry, and published in its Journal.

The Gilliams Service

Cutting up a sulphur bottom whale on the Pacific Coast

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Upper jaw of a whale, showing "whalebone"

Ocean Venison

Whales to Furnish Additional Supply ot Meat

To take the place of our rapidly decreasing beef supply a new and much cheaper meat which can be sold for about 12 cents a pound retail has appeared in some of our markets. It is called ocean venison, which is camouflage for whale meat. The Food Administration and the United States Bureau of Fisheries highly endorse it and hope it will be soon generally used. By the Eskimos and Japs it has been eaten for years and regarded as a great delicacy. It has been tried in some of our Pacific Coast cities and New York and found highly palatable both fresh and canned, and it is now likely that it will only be a few months before this food will be as much of a commodity in our butcher shops as beef or mutton.

The only wonder is that it took the war, and the consequent scarcity of all kinds of meats, to induce us to come to it as most of those who have eaten it have decided that we should have been using ocean venison on our tables for many years as an efficient and economical food.

The trouble is that tradition and prejudice have controlled our dietary habits so long that most of the abundance of the seas has been ignored. Just as the men of Plymouth Rock took a chance on the mussels, and thereby were saved from starvation by the treasures found in the sands, so we of the nation which they helped to found may reduce our living costs by avenging Jonah.

Quite recently there was held in New York City a dinner attended by several hundred people. At this feast the principal dish was ocean venison. Among the guests at this repast was Admiral Peary, who was invited because he is regarded as the most distinguished connoisseur of whale flesh in the United States, having frequently lived upon it for months at a time during his Arctic explorations.

Admiral Peary says he thinks that, all things considered, he rather prefers a whale meat steak to the ordinary beef variety, and at the dinner he urged that no time be lost by the National Food Administration in introducing the meat in every part of the country.

Dr. H. M. Smith, chief of the United States Bureau of Fisheries, highly endorses it as food as Government tests and experiments have fully borne out the fact that whale meat is not only edible, but also possesses from 3 to 4 per cent. more protein than beef and of its nutritive part 98 per cent. can be digested.

Since the breaking out of the war and scarcity of food the world over, Denmark has been using the flesh of sperm whales extensively in feeding her soldiers, and Norway has also made a similar use of it. But previously to the war the Japs and the Eskimos were about the only people alive to its possibilities and to hunt the whale for food.

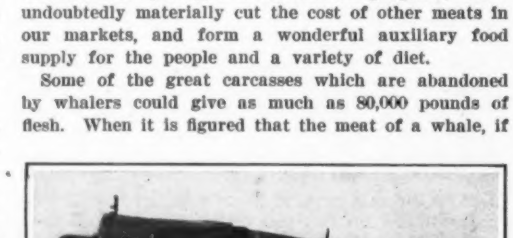
The Eskimos take much of it into their systems, although they prefer the blubber because it is more heat making. The Japanese have held whale meat to be a delicacy for years. So perfect are their methods of disposing of the great bodies that hardly a particle is wasted. To them the whale fisheries are as the great cattle ranches of the West to the American.

As a matter of fact, whale flesh is the beef and mutton of the Japanese, and when it is considered that one huge body gives as much food as 100 cattle, it will be seen what an important industry, in keeping down food costs, whaling is to these people.

The greatest contrast between whaling among the Japs and other people of the world is in the disposal of the bodies. The Japs alone know how to get the full value of their catch. American and other whalers take only a small portion of the whale, using only parts of the blubber and the bone.

If American housewives can be persuaded to use whale flesh on their table extensively the thousands of tons of perfectly good meat which are now tossed into the ocean yearly, if saved for food purposes, would undoubtedly materially cut the cost of other meats in our markets, and form a wonderful auxiliary food supply for the people and a variety of diet.

Some of the great carcasses which are abandoned by whalers could give as much as 80,000 pounds of flesh. When it is figured that the meat of a whale, if sold for food, should be worth, even at a very low price per pound, from $2,500 to $3,000, it will be seen what an immense amount of money has been thrown away during the past fifty years by the whalers of our Pacific coast stations where the blubber is rendered for oil and bones cleaned for market, but the flesh discarded, or only to a limited extent converted into fertilizers.



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Formerly whales were captured by a harpoon, thrown by hand from a boat. Now an enlarged harpoon is shot from a howitzer on the bow of the steamer

As these stations take a considerable number of whales yearly, each furnishing several tons of excellent meat, it is evident that a tremendous quantity of a valuable food has been going to waste. Not so with the Japanese whalers who prepare the whale meat in a number of ways for market. The people of Nippon consider the flesh of the humpback whale caught in the Japan Sea and along the western shore of the island, a very great dainty.

Roy C. Andrews, the well-known naturalist, tells how, while on a recent hunting trip for the gray whale, when he brought ashore on the coast of Korea two excellent specimens of these whales to preserve the skeletons, a crowd of hungry-looking Koreans eagerly watched his operations hoping to find an opportunity of stealing enough meat to make a "whale stew." Finally the naturalist picked out half a dozen of them and told them to go ahead and clean the meat attached to the bones.

So eager were the natives for the food that when the task was completed the skeletons needed no further cleaning. Everything was gone except the bones, and these doubtless would have gone too, if the naturalist had not kept a close watch on the workers.

Whales, it must be remembered, are mammals, like cattle and sheep, and their flesh is "meat," not "fish." In texture and appearance it resembles beef, though the color is darker red. Its taste is rather gamey and that is why it has been called ocean venison. Those who object to the gamey taste, for there are men and women who do not like the rare and racy savor of the flesh of the creatures of the wild, can overcome this flavor by a few applications or rinsings of water in which salt or soda has been dissolved, or by parboiling. But at the recent dinner in New York the guests took their ocean venison straight and relished it.

The feeding habits of the whale are very cleanly, as it lives mostly on small organisms which, in many species, are strained through the meshes formed by the baleen or so-called whalebone in the back of the mouth. As the throat of most whales is very small, despite the tactics which one of the race is supposed to have employed toward Jonah, he is not in search of large masses of matter, like the shark, which may become under certain conditions a scavenger of the sea. The meat and the blubber of the whale have a cleanly and sweet odor.

The fact that whales are only captured in far Northern waters, at a great distance from some of our inland cities and markets, does not matter in the least in these days of advanced refrigeration systems. Pacific fish, in excellent condition, are constantly on sale in New York and other Eastern and Southern markets, and as for beef we now ship it all over the world.

While mutton is sent from Australia to England regularly, whale meat undoubtedly could be shipped anywhere if handled under similar conditions.

It is figured that the fresh or refrigerated whale meat can be brought east from the Pacific and sold at 12½ cents a pound retail, while the canned product, packed in tins, should cost 18 cents a pound at the grocer's. In these days, when prime beef is bringing 35 to 40 cents a pound in choice cuts, and 40 cents is asked for chicken, there may be found good reasons of economy for the use of ocean venison.

Several canneries of whale meat established on the Pacific Coast, principally in Seattle and in that neighborhood, follow in methods the Japanese canneries, in which the process of preserving the meat has been perfected. The Americans are using at present the flesh which comes from the big back muscles, although there is no reason why practically all the meat should not be employed.

On a 65-ton whale it is said there is likely to be 40 tons of lean meat available for food, the balance being bone and blubber.

The meat is steamed and packed in one-pound cans, and then cooked some more before the cans are sealed and labeled. It darkens as it cooks. When a can is opened the meat disclosed has the appearance of the larded venison by which connoisseurs set store. It does not in any way resemble the corned product.

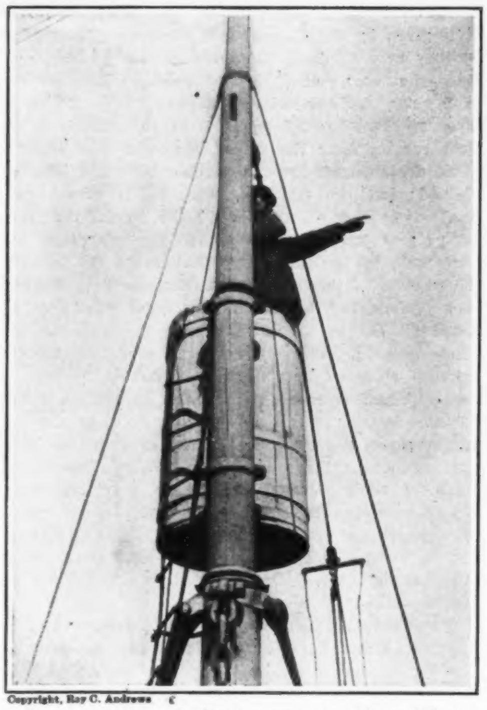
Tinned whale meat may be served in all forms in which beef is adapted to the human palate. It can be converted into soups and curries and roasts; and when properly disguised its origin can be completely concealed. There is no fishy flavor about it and no suspicion even of the strong blubber-like taste which those prejudiced against it might expect to discern. In fact the percentage of fat is very low, so that the meat might seem almost dry as compared with other flesh.

It has been supposed for years that the whaling industry has disappeared from the seas. Such, however, is not a fact as, although New Bedford does not send its time-worn craft in quest of whales as it did of yore, whaling still continues.

The canny Scotch are looking for the monsters within the Arctic circle, while in the Far East the Japanese have developed whale hunting into a great industry.

As far as whaling on our Pacific coast is concerned, it might well be called an infant industry which is doing fairly well. With the demand for whale meat growing there seems no reason why eventually there should not be a good supply brought into the markets of the United States. While American whaling men have heretofore consigned the major part of their catch to the sharks they have usually kept a small percentage for themselves.

On whale ships this meat is made into what are called whole meat balls. In addition to spices, a little salt pork is cut up very fine and mixed with the meat, and the balls are then fried. When cooked they are about as large as a medium-sized grapefruit, and a man who cannot eat at least two of them is considered in proper condition to patronize the medicine chest.



Copyright, Roy C. Andrews

Crow's nest or lookout on an American Whaler. When the vessel is on the hunting ground a man is constantly stationed in the nest and his cry "There she blows" is eagerly awaited by the sailors on the deck below.

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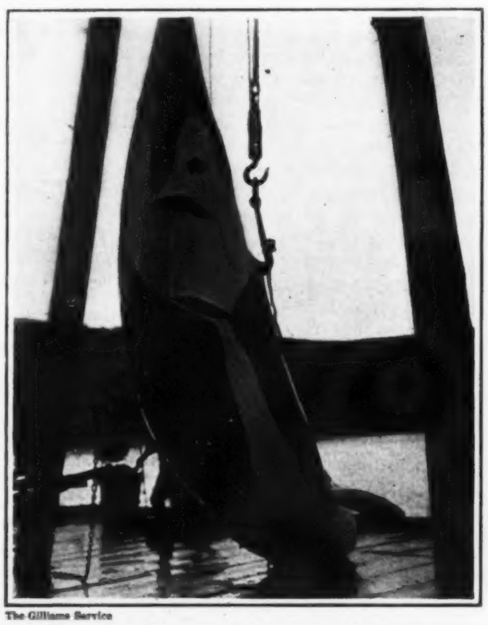
A good catch by a steam whaler



The Gilliams Service

Part of a whale suspended by a derrick for convenience in butchering

Alloys to Withstand Internal Air Pressure

Density and strength are the two qualities that go to make up a metal suitable for the retention of air or other gases under pressure, and while strength may be secured through proper design, density is elusive. This leads us to the conclusion that there can be no hard and fast rule whereby density can always be obtained, probably because of the many variables that enter into the process. To enumerate some of these variables, there is the design of the article to be cast, the design of the pattern with reference to its position in the flask, composition of the alloy, the treatment of the metal in the furnaces, and the temperature of the metal when being poured.

The design of the article to be cast has a very important bearing upon the ultimate success of the casting. The designer should bear in mind the desirability of having all cross-sections of approximately equal thickness in order to prevent draws at heavy portions. If this is not possible, access to all large sections should be allowed for the use of chillers to prevent such draws. If the cored cavities are large the cores will themselves act as chills. Fillets should be as small as possible.

In laying out patterns, the patternmaker must be governed by several things. He must know what chillers are to be used, so that large chilled surfaces may be placed in a vertical position in order to prevent the metal kicking off these surfaces. He must know what parts are to be clean, such as valve-seats, etc., and to what parts loose sand may be allowed to flow if any be found in the mould. Such unimportant parts should be placed high in the cope so that the loose sand will flow to them on top of the metal. A clean mould, however, is absolutely essential to good tight castings. An exceptionally clean casting may be obtained by gating it from another casting which will itself take all the dirt.

On the use of chillers might be stated as an almost universal law: Use chillers on all enlarged sections in close proximity to smaller sections and connected thereto. If the sections are exceptionally large, use a sinking head on top of the large sections. Gate the moulds with a heavy upright pouring gate as near to the pattern as possible. The gate leading from the pouring gate to the pattern should be made large at the pouring gate, and then reduced sharply into the pattern. If it is large where it joins the pattern, in all probability it will show a draw in the casting at the gate. As a rule, it is better to gate in a light part of the casting than in a heavy portion. If a sinking head be used it should be placed on the heavy part.

As regards the alloy to be used, the following compositions have been tried and found satisfactory for the purpose intended:

	No. 1.	No. 2.	No. 3.
	%	%	%
Copper	72·50	82·00	83·00
Tin	1·75	7·50	11·50
Zinc	19·25	4·75	4·00
Lead	6·50	5·75	1·50

No. 1 is used for ordinary castings, such as cocks, pistons, bushings, etc. This alloy is easily machined, but is not intended for use with very high pressures. No. 2 and No. 3 alloys are intended for use with high pressures, and are harder to machine in proportion.

The treatment of the metal in the furnaces is of vital importance. If proper allowance for oxidation of zinc, etc., is not made the alloy intended will not be produced. Furthermore, the metal must be taken from the furnace as soon as it reaches the proper heat, for if allowed to soak in the furnace it will take up gases, and the castings made from it may be porous. In certain packing-ring mixtures we consider this item so important that we use an alarm clock to ensure the metal being poured off at exactly the proper moment.

The temperature at which the metal should be poured into the moulds is important. If poured too cold it is almost impossible to obtain solid castings, especially at the gate. On the other hand, if poured too hot, the castings may be porous throughout. Great care must be taken to see that no aluminum gets into the mixture, as a very small percentage of it will cause the castings to leak. Antimony and iron will do the same, but not to so great an extent. Aluminum has a very peculiar action on the metal. The castings will look solid and will not show a draw, but when put under pressure will leak all over. It is one of the most dangerous metals around the brass foundry. Antimony does not act as quickly as aluminum, but has about the same effect if used long enough in the mixture. With a small percentage it may seem to do no harm, but if used until it is mixed with all returned material, such as turnings, gates, etc., the castings will become porous.—*By* Mr. S. D. Sleeth, *American Institute of Metals.*

Stone Implements Found in Australia

Some exceptionally large stone implements discovered in 1887-88 near the Johnstone River, on the Pacific coast of Queensland, are described in a recent issue of *Man* by Mr. H. Ling Roth. The materials from which they are made are an altered diabase, argillaceous and micaceous grit, and an arenaceous shale. One implement measures 16.5 cm. by 10.9 cm. by 2.9 cm. Dr. Walter E. Roth, who made some inquiries regarding them, states that at the present day such stone axe-heads are not used—in fact, no stone axes are used. They seem to have been procured from quarries, one about ninety miles from the scene of the discovery. Dr. Roth found, in the neighborhood of Boulia, an axe-head measuring 9 inches in its greatest diameter—considerably larger than any in the collection now described. These appear to be the largest dressed stones higherto found in Australia, but the Bankfield Museum possesses a similar implement from Lifu, Loyalty Islands, formed of impure jade. It is not so large as some of the big New Caledonian stones fastened at right angles to a handle by sinnet passed through two holes in the stone.—*Nature.*

Extracting Vaporous Constituents of Coal Gas

At a recent meeting of the London section of the Society of Chemical Industry, Dr. R. Lessing read a paper on "A New Method of Extracting Vaporous Constituents from Coal Gas." He said, so far it had only been used for research purposes, but there were hopes that before long it would be made available on a commercial scale, so that it could be used on gasworks in the ordinary way both for the purpose of extracting benzole and as a check on gas manufacture generally. The principle of the process is that of a dry scrubber filled with solid absorbent material, which will strip the benzole from the gas without the employment of running wash oil, and from which the volatile products could be recovered by steam distillation *in situ*. At first it seemed that crushed pitch would serve the purpose of the absorbent material, but it was found that its viscosity decreased to such an extent by the absorption of the solvents from the gas that it began to run after a while, and was liable to consolidate and block the passages of the apparatus. Finally a rigid material was decided upon, and broken firebrick was found to serve the purpose admirably.

The Ether Ball

By N. Johannsen

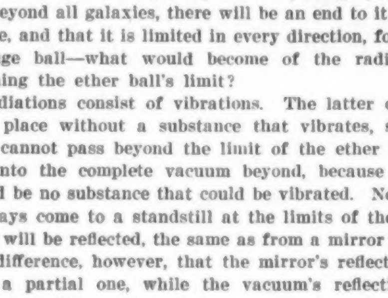
WHAT becomes of the heat radiated by the sun? Only 1/2 700,000,000 of it strikes the earth, and a few hundred times more than that is allotted to the planetary family. The planets, on their part, do not retain the heat, thus received, but send it out into space. So the whole of the sun's radiations, enormous as they are, pass out into fathomless space. Just so the radiations of all other stars. They are carried out by the ether into the unlimited depths of space. Are they actually lost, and do they go out of existence? Hardly.

Space is unlimited. But is the mass of ether also unlimited? Hardly.

If we could imagine that the mass of ether is limited, the same as our atmosphere; and that, though reaching far beyond all galaxies, there will be an end to it somewhere, and that it is limited in every direction, forming a huge ball—what would become of the radiations reaching the ether ball's limit?

Radiations consist of vibrations. The latter cannot take place without a substance that vibrates, so the rays cannot pass beyond the limit of the ether ball—not into the complete vacuum beyond, because there would be no substance that could be vibrated. Nor can the rays come to a standstill at the limits of the ball. They will be reflected, the same as from a mirror; with this difference, however, that the mirror's reflection is only a partial one, while the vacuum's reflection is complete, with no loss whatever.

The ether ball must not necessarily have a true spherical shape. It may be a lens, or even have irregular outlines; that does not matter, so far as reflection is concerned. Suppose a protuberance of the ether



substance to exist at some place of the outskirts, and the ray X to strike the surface at Z. It will be reflected twice, or even oftener, with the protuberance, but finally will return into the ether ball, no matter in what direction.

There it will be steadily on the go, to and fro, traversing the ball from one end to the other, until it strikes either a stellar body or one of those "worlds in the making," about which Professor See has been, writing as consisting of cold nebulae. When absorbed by one of these, the ray is changed into heat again, which remains with the nebula until, at some later period, the new world, emerging from that nebula, begins stellar life and sends out heat rays of its own into the universe—whereupon the heat, brought in by that particular ray X, may be set free again, to resume its journeys to and fro in the ether ball until it is once more absorbed by some nebula.

Will it take a long time before the ray finds another resting place? Most likely it will. To traverse the ether ball from one end to the other may take a million years, and it may have to perform many trips before it strikes a nebula and meets absorption. On the other hand, the universe harbors an immense quantity of such nebulae, and as their mass is exceedingly thin, they possess a very large surface by means of which they capture the vagrant rays—a surface very large in proportion to the mass contained within it. It has been computed that the aggregate of their surfaces is hundreds of millions times larger than the aggregate of all stellar and planetary surfaces.

The "vagrant rays" must not be understood as consisting of *isolated* rays, occurring here and there. The ether ball is full of them. The rays sent out by the sun at any one moment will spread out in every direction and will successively pass through every point of the ether ball, excepting only the places where they have been intercepted by some nebula, star, or planet. Thus, almost every cubic inch of ether is constantly being traversed by rays coming from the sun, and not only from our sun but from all other suns, stars, and planets as well—and not only from these, but also from the reflections of the ether ball's outskirts. In consequence, all exposed surfaces within the universe receive a uniform supply of celestial radiation. True, this supply is quite small, and hardly measurable; but it is there, all the same.

Have we any evidence of its existence? We have. It is given by the fact that in clear moonless nights, be they ever so dark, there are certain places in the skies, the "coal sacks," decidedly darker than the rest of the sky. This shows that the great bulk of the sky gives us *some* light, faint as it is, in addition to the light coming from the stars. Why the coal sack regions exhibit such intense darkness has not been explained; but the fact that they do it proves most conclusively that the rest of the sky, by far the greatest part of it, is constantly shedding *some* light and heat upon us, making every particle of the universe the receiver of this steady radiation—perhaps the coal sack regions, too, but not in a way visible to us, because all the rays received there are absorbed and none reflected. Possibly the coal sacks represent a primary stage of "worlds in the making," where all the radiation received is used for developing a new nebula—this being the reason why no rays are sent out or reflected by them, and why they are so dark; the luminous nebular state being only a subsequent stage of their development where they are under strong one-polar electrical excitement, strong enough to overcome the mutual attraction of the nebula's parts and to bring about a certain repulsion which causes branches or arms to flow out at opposite sides of the nebular mass.

Were there no such non-luminous primary stage we might well ask the question: Inasmuch as even the faintest nebula is brighter than the sky, how could it acquire its own luminosity from outside, i.e., from a source of light weaker than itself? The greater luminosity of all nebulae clearly shows that they radiate more light and heat *to* the sky than they receive *from* it. Now we know that radium is doing just such a thing: it radiates light and heat and at the same time receives and absorbs heat rays from the surroundings, being always about three degrees colder than the surrounding temperature; but the circumstances are quite different, radium disintegrating and thereby transforming its own latent heat into free thermal heat, whereas the dark nebula is bound to do the reverse—acquiring the free radiation heat of the ether ball and transforming it into latent heat, of which it has to store up a stupendous amount before it can enter the luminous stage. If we consider the immense amount of heat radiated by the sun from its early nebular state down to the present (not only the heat derived from shrinkage but also the much larger amount due to disintegration of radium and other substances) we may get an idea of the amount of heat which had to be fed into the dark nebula before it could enter the luminous stage—all of which heat had to be derived from the radiant heat traversing the ether ball—small as it is in intensity, yet almost boundless in aggregate amount.

According to the foregoing, two important points may be deduced from the existence of the celestial "coal sacks"; *first*, they prove the outskirts of the heavens to be slightly luminous, for without such luminosity the whole sky would be as black as the coal sacks; *second*, they prove the coal sack regions to be stronger absorbents of light and heat than the rest of the sky; which greater absorption can only be explained by the heat requirements in building up the first stages of a new nebula. Maybe that the coal sacks will gradually shrink down to a luminous stage, whereupon fresh coal sacks will appear elsewhere.

The assumption of an ether ball may prima facie seem a matter of grave doubt. It will at once raise the questions: what should compel the ether to remain within the limits of the ball? Why should it not follow the tendency of all gases, unless confined, namely, the tendency *to expand?* Why should it not expand further and further, out into the vacuum beyond? Is there any power able to check that expansion, and to keep the mass of ether within limited bounds? Such a power exists, indeed.

Hydrogen has for a long time been considered a "permanent" gas, until, under the influence of great pressure and intense cold, it was reduced to the liquid state. By sufficiently decreasing its temperature it can be liquefied without applying pressure; in fact, not only liquefied but solidified. This law applies not only to hydrogen but to helium, coronium and to any other gas. Why not to ether as well?

The temperature of the ether ball is not the lowest possible temperature existing in space; beyond the ball it is *lower*. Will the ether's heat radiate into that colder vacuum beyond? It cannot do that, because radiation means vibration, and in trans-ethereal space there is no substance that can vibrate. The heat, therefore, cannot leave the ether ball—radiation to the outside being prevented in a similar way as in the thermo-bottle or in the electric flat iron. But what restrains the ether itself from darting into that vacuum? Because that would mean expansion, and the ball's ether is already expanded to the limit where any further expansion (identical with further cooling) would mean condensation—either to the liquid or to the solid state.

Here we arrive at the physical basis for the existence of an ether ball: the ether's temperature is not the lowest that exists in space, but it is the lowest than can keep ether in a gaseous condition, and this naturally puts a limit to further expansion.*

There are three chief requirements for "worlds in the making"—heat, gases and solid substances. The source of the latter has been explained by Arrhenius, See and others: the vapors of such solid substances are thrown out, at times of violent action, by the sun and the stars, out to distances where the surroundings are cool enough to allow those vapors to congeal into minute particles, and these are carried forward into the depths of the universe by radiation pressure. To some extent the solid substances seem to be supplied also by old worlds bursting up. As to the source of the *gases* (the second requirement), it seems the ether ball contains them and supplies them—in fact, the so-called ether may consist of nothing but those gases, especially helium, hydrogen, etc., highly attenuated. The working of these two gases is very peculiar; they are abundant in new worlds (and presumably also with worlds in the making) but disappear when the worlds are getting old and cold—disappearing into ethereal space. (The idea that ether consists of nothing but highly attentuated hydrogen, etc., has been disputed because the vibrations of the hydrogen's molecules are said to be too large to transmit the very small vibrations of light; but then, have the ether's molecules any smaller vibrations? Again, if cold hydrogen absorbs the rays of incandescent hydrogen, but not those emitted by other incandescent substances, does not that clearly prove the hydrogen, *and not the ether within the hydrogen*, to be the transmitter of the light coming from those other substances?) As to the source of *heat* (the third requirement), this has been assumed to come chiefly from contraction, the same as is the case in the sun—without considering that only gases, when contracting, yield a large supply of heat, whereas the solid substances, such as build up new worlds, have already contracted to the last stage. Also the mutual collisions of these small solid substances, taking place here and there, have been thought of as a great source of heat, but the heat of such stray collisions will quickly be radiated and does not count. The real, inexhaustible source of heat must be found in the rays filling the ether ball, though the manner of utilizing this heat, in building up new worlds, remains for future investigation, the same as so many other mysteries of the universe.

Storage Batteries or Reversible Batteries?

A WRITER in the *English Mechanic* suggests that the word storage applied to batteries is not correct, as electricity is not stored up, but rather a quantity of the active elements, and it is the chemical changes that ensue which cause a current to flow when discharging. He suggests that "reversible" would be a better term than "storage."

*The foregoing assumption may not seem to agree with Gay-Lussac's well-known experiment, from which he concluded that air, or gas, when expanding into a vacuum, is not cooled by the operation. This conclusion, however, has not been confirmed by subsequent experiments which proved that the expanding air, contained in vessel *a*, is actually cooled, whereas the vacuum vessel *b*, into which a part of *a*'s air flows, is heated. It is heated because the kinetic energy of the inrushing air is transformed into heat. No such heating would materialize on the part of the ether ball's outskirts, if these were to expand—not into a closed vessel, but into open, unlimited space—there only the cooling effect of the ether's expansion could take place, without a corresponding heating effect on the part of the ether expelled from the ball. Another force, tending to check unlimited expansion of the ether ball, must be found in the principle of "gaseous cohesion" the existence of which has been proved by many experiments. Such gaseous cohesion is negligible under ordinary temperatures (which counteract it) and under ordinary pressures (which entirely eclipse it, at ordinary temperatures), but it becomes more and more important the lower the temperature of the gas. The nearer a gas comes to that degree where it assumes the solid state, the less will be its expansive force and the greater its force of cohesion until, when reaching the liquid or solid state, cohesion rules supreme. Just so with the ethereal substance—at some very low temperature the increasing force of cohesion and the diminishing force of expansion will check further expansion.

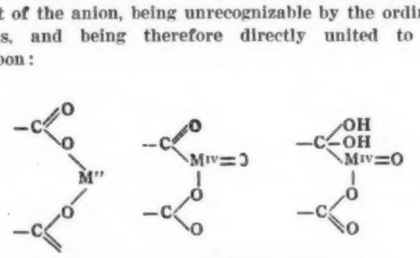
Problems Bearing on Residual Affinity*

By Spencer U. Pickering, M.A., F.R.S.

The great majority of known inorganic compounds cannot be represented without attributing to the constituent atoms valency values higher than those commonly ascribed to them. The combination of molecules with each other is sufficient proof that the atoms composing them must possess residual affinity, and therefore additional combining capacity; for the properties of any mass of matter can only be those of its constituent particles. The residual affinity of an atom, A, may be insufficient to attach to it more than one atom, B, but might suffice for the attachment of another B atom if this were already combined, and had only residual affinity available; hence the higher valencies can be expected to come into play only in the formation of complex or molecular compounds, the constituents of which will be united with comparative feebleness.

Oxygen in water, nitrogen in ammonia, and sulphur are conspicuous in forming molecular compounds, and also in polymerizing, as is shown by the phenomenal magnitude of the heat absorbed by them on rise of temperature or change of state. On the other hand, carbon and hydrogen appear to have little or no residual affinity, and hence have fixed valency values. As a consequence, compounds of these two elements can be built up in a way that is impossible in other cases. This is also the reason why the substitution of CH_3 for H in an organic compound, or of any other single atom such as Cl or O, evolves practically a constant amount of heat, whereas the introduction of a second Cl or O, since it admits of the residual affinity of the two atoms coming into play, results in a larger heat development.

The metallo-compounds investigated by the author necessitate higher valency values for many metals than those generally exhibited by them. They are isomers of the normal organic salts, the metallic atoms forming part of the anion, being unrecognizable by the ordinary tests, and being therefore directly united to the carbon:



Normal. Metallo-compound anhydrous. hydrated.

They are obtained as emulsions, either by double decomposition or by adding alcohol to a freshly prepared solution of the compounds. They are much more soluble than the corresponding normal salts, and gradually change more or less completely into the latter, with, in some cases, a considerable alteration in color intensity. In some cases where the change is not too rapid they may be obtained as scales or glass on evaporating their solutions. Over forty pairs of such isomers have been obtained containing thirteen different metals, and five different acids; whilst the color intensity of the copper salts of other organic acids renders it practically certain that similar isomers must exist in all such cases.

Their preparation by double decomposition with the organic salts of sodium implies that these too must exist as such in solution, and the sodi-tartrate and sodi-citrate they have been isolated in scale form.

Alkalis or metallic oxides may take the place of the H_2O in the hydrated metallo-salt, and many such compounds have been isolated, some of them being isomeric with ordinary basic salts, from which, however, they differ by being soluble. But the oxygen atom may be displaced by any basic radicle, hence a salt or an acid may combine with a metallo-compound, M_2R'', or H_2R'', taking the place of H_2O; such compounds may be termed metallato-compounds; e.g., sulphato, carbonato, etc.; many of the cupricarbonates originally isolated were carbonato-cupricarbonates. If the basic and acid radicle in the salt combining with the metallo-compound are the same as those in metallo-compound itself, the product will be isomeric with the simple metallo-compound, and also with the normal salt.

Metallato-compounds are intermediate in solubility between these two; they form jellies or gelatinous precipitates, consisting of minute membranous particles. The formation of them constitutes one of the four or five stages (some of which are often unrecognizable) occurring in double decomposition. When barium chloride is added to concentrated potassium tartrate solution, an emulsion of the bari-tartrate is first precipitated; this rapidly redissolves forming an ato-compound with the potassium tartrate present, its formula being BaT,K_2T; then a bulky precipitate of the ato-compound, BaT,BaT, forms, which after several hours begins to redissolve, and the crystalline normal salt separates from the liquid. With the citrate more than one mixed ato-compound, $Ba_3Cit_2x(K_3Cit)$, is produced, some of which are sparingly soluble.

Such metallato-compounds, since they contain the group OK, not derived from an acid hydroxyl, must be alkaline, and it is owing to their presence in solutions of organic salts that these are alkaline. Such alkalinity cannot be explained by hydrolysis, for it is found that litmus is equally sensitive to acids and alkalis, and hydrolysis would liberate equivalent proportions of each.

The neutralization of an alkali by an acid is but a special instance of double decomposition, and follows the course indicated above, but the ato-compound formed, instead of being alkaline, will be neutral, except for a slight acidity due to partial hydrolysis. Definite compounds with acids have been isolated in several cases where the metal was one of the metals of the alkaline earths, and where it was one of the alkali metals proof of their existence has been obtained by acidimetric methods. It is owing to the formation of these neutral compounds of acids with organic salts that exact titration of organic acids is generally impossible. The proportion of acid combining with the salt in solutions of decinormal strength varies from 0.3 to nearly 4 equivalent. A curious result of such combination is that the presence of potassium citrate, etc., prevents the evolution of carbon dioxide when an acid is added to a carbonate.

Ato-compounds containing different acid radicles have been investigated. Evidence as to the existence of $2K_3Cit_2(K_2SO_4)$ has been obtained and it is owing to the formation of this (or analogous compounds) that the presence of an organic salt interferes with the determination of sulphuric acid, barium, etc., by precipitation. A barium salt added to the above compound results in the formation of a small quantity of a gelatinous or membranous precipitate, which is evidently an ato-compound, but contains only the elements of barium sulphate, and must be sulphato-sulphate. Other evidence as to the possibility of inorganic salts existing in the metallo condition has been obtained.

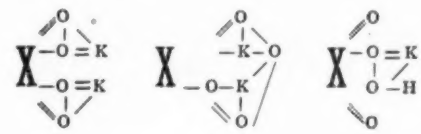
With organic compounds acidity is dependent on the presence of hydroxyl side by side with a doubly-linked oxygen. It is inconceivable that a constitution which is essential in this case should not be essential in any other; it is equally inconceivable that this doubly-linked oxygen, if essential to acidity, should play no part in neutralization. The acidity of the haloid acids is known to depend on the presence of water; to admit of the true acid containing the group XOOH, the elements two molecules of water must be present, and, as a result, the halogens must be heptads, a value already assigned to them on other grounds. Similarly, sulphur, etc., will sometimes have to be octads, and phosphorus, etc., nonads. That a doubly-linked of oxygen is present in all acids seems to be proved by the fact that all the so-called neutral salts of the alkali metals, and of the metals of the alkaline earths, are really alkaline, just as are the salts of organic acids, though in a much feebler degree; the same explanation must apply in both cases, and this involves the presence of some of the salt in the form of a metallato-compound, and hence the presence of X=O. The alkalinity of these salts is independent of the method of preparation and purification, and is of the order of $5\times10-4$ MOH for a grm.-equivalent of the salt.

Since carbon is incapable of assuming a valency higher than 4, carbonic acid can be monobasic only; the supposed dibasic acid, H_2CO_3, is unknown, and the compounds derived from it have certainly no claim to be considered as true salts—the product of the *neutralization* of the acid by an alkali—for with alkali metals they are strongly alkaline, and the heat of their formation is much below the normal. All the other seven cases in which the heat of neutralization is abnormally low are similar. Ortho-phosphoric and arsenic acids must be dibasic only, unless we assign an unacceptably high valency value to phosphorus and arsenic, and the heat of neutralization shows them to be dibasic, the displacement of the first two hydrogen atoms evolving the normal amount of heat, whereas that of the third evolves little more than half this quantity, and the resulting products are strongly alkaline.

The accepted neutralization equation—

$$XOH+HOM=XOM+HOH$$

represents a case of double decomposition, which should therefore be reversible, and in which the heat evolution should be 8,900 cal. (the heat of condensation of a molecule of water), since the chemical interchange of atoms from combination with one oxygen atom to combination with another should evolve no heat. The fact that the actual evolution is 13,743 cal. shows that some affinity not represented by the equation must become satisfied. This must be the residual affinity of the doubly-linked oxygen atom, which comes into play on the substitution of hydrogen, which possesses no residual affinity, by a metal which possesses such affinity. With these affinities operating, a normal salt of a dibasic acid will be represented by I.,—



I. II. III.

and the corresponding metallo-compound by II.; this latter, as will be seen, is now represented as the less saturated compound of the two, which it evidently is, since it changes spontaneously into the normal salt; this agrees also with the heat of neutralization values, for in the case of organic acids, where the metallo-compounds are the main products, the values are 2 per cent. lower than with inorganic acid. Another remarkable feature of the heat of neutralization, which is quite inexplicable on the dissociation theory, is explained by these views, namely, that the total heat of neutralization of dibasic inorganic acids is normal, but that the addition of the first equivalent of base develops 18 per cent. more heat than that of the second. For mula III shows that the acid salt need not be, as it thus certainly is not, intermediate between the neutral salt and the acid. But with organic acids the heat developed on adding successive equivalents of base should be, and is, almost constant, for no acid salt is formed; the first equivalent of base produces the metallo-compound, which unites with the rest of the acid left to form the neutral metallato-compound, and the second equivalent converts the acid in this into the normal salt, this remaining combined with the metallo-compound in the form of the slightly alkaline metallato-compound.

In many cases there is a development of heat, often considerable, on adding alkali in excess of that required to saturate an acid, and the combination of alkali with normal salts have further been established by alkalimetric methods; the combination results from the conversion of MO—X=O into MO—X=$(OK)_2$, analogues of basic salts. Taking the difference between the heat of neutralization of inorganic and organic acids as being due to the fact that the normal salt is almost the sole products in the former case, and the metallato-compound in the latter, a rough estimate may be obtained of the heat developed in satisfying the residual affinities concerned, and, applying this to Formula I, the value for the normal heat of neutralization should be 12,419 cal., which is in quite as good agreement with the observed value of 13,743 as could be expected under the circumstances.

*From a paper read before the Royal Society, and reported in the *Chemical News*.

The Fourth Colorless Sensation

A contribution by Sir William Abney to the "Proceedings" of the Royal Society, discusses a point of considerable interest in connection with theories of color, namely, the "colorless" sensation which is experienced when an image of colored light is received on the peripheral region of the retina. When an image of a faint light of this kind is so received, even though itself consisting of a pure monochromatic color, it appears white, or rather gray. This effect has therefore been termed the "fourth colorless sensation," supplementary to the red, green and blue elements constituting a trichromatic system. Sir William Abney also refers to the theory of the rods and cones on the retina, which, we believe, is now accepted by physiologists in the form originally presented, but which certainly helps to explain many pehnomena otherwise inexplicable. According to this theory the cones, with which is associated the perception of color, and which are located mainly at the center of the retina, operate chiefly at relatively high illuminations; while at low illuminations vision is taken up by the rods, which occupy the peripheral region, respond chiefly to faint light, and cannot discriminate color On this supposition the fourth colorless sensation should not be experienced when light falls only on the fovea. Yet the author finds that in some cases the effect does exist.—*The Lancet.*

Increasing the Evaporation in Steam Boilers*

A New Thermal Principle in the Boiling of Water

By Carl Hering

IT is well known that water may be boiled in a cup made of ordinary paper; also that a postage stamp may be pasted on the flame side of a metallic vessel in which water is being boiled and although the flame plays directly upon this stamp it will not be charred. It is perhaps less well known that when a second or third stamp is pasted over the first one, the outer ones will char. If there is a blister in a single thickness of the paper, that blister will char.

GAS FILM ON FLAME SIDE OF VESSEL

The interpretation of this is that when very hot gases, like those of a flame, impinge upon the outside surface of any water-boiling vessel, which is constantly maintained at a far lower temperature by the water on the other side of it, a thin film of gas forms on the flame side of the surface which offers an enormously high resistance to the passage of heat through it; its specific resistance appears to be far greater than that of thermal insulators, yet all the heat which flows usefully from the flame to the water must traverse it; this film is therefore a very great obstruction to the flow of heat, and this method of heating is a very irrational one, although it is the usual way. The thermal resistance of the metallic walls of the vessel is so small in comparison that there is no appreciable gain in the heat flow by using copper tubes in a boiler in place of iron ones, even though copper conducts heat much better than iron.

If the temperature of the flame is taken at about 1,350 deg. C. (2,462 deg. F.) and that of the water is 100 deg. C. (212 deg. F.), there is a fall of temperature of about 1,250 deg. C. through this film, which appears to be only about 0.005 in. thick; this means an extremely high thermal resistance, so high that it is a question whether it is a true resistance; but as it certainly acts like one, it may at least be here referred to by this term.

If this high-resistance film could be broken down, the heat would flow more rapidly from the flame to the water, which means that the water could be boiled faster or that the boiling vessels, like steam boilers, could be made smaller for the same steaming capacity; also that the losses of heat would be reduced, for if a given quantity of water could be boiled twice as fast, for instance, with the same flame, the heat losses will be reduced to a half, as they take place during only half the time.

One way to reduce the resistance of this film is to use a blast flame, which seems to mechanically carry away part of the film; a strong blast flame directed against the aforementioned postage stamp will char it; but this method is not generally practicable. The usual way is to increase the surface exposed to the flame, but doubling or trebling this surface while using the same flame does not necessarily double or treble the heat flow; if the volume of the flame is then also doubled or trebled, the amount of boiling will, of course, be increased proportionately, but this simply means doubling or trebling the whole boiler; this increases the quantity of heat transmitted but not the rate; nor does it increase the efficiency very much.

By studying the nature and properties of this high-resistance film the writer found that its resistance diminishes very rapidly when there is less difference of temperature between its two sides; namely, between the flame and the metal. In boiling molten zinc, for instance (about 950 deg. C.), instead of water, the resistance of this film would be very greatly reduced. It is probably also very slightly less in high-pressure boilers in which the temperature of the water is higher.

It seems to be analogous to the case in mechanics in which a heavy weight struck by a sharp blow will move only slightly, but when the same energy is exerted on it less violently, the body will be moved more freely by it; the resistance of the body against being moved (due to its inertia) becomes greater as the suddenness of the blow increases. Our present method of heating water may be said to be analogous to moving a heavy car by applying sharp hammer blows at the rear, in which case its inertia acts like a high resistance; this analogy, however, is only approximate.

ESTABLISHING ARTIFICIAL THERMAL RESISTANCE

As the temperature of the flame is fixed, and it would be inadvisable to reduce it, and that of the water cannot be raised, there remains only increasing the temperature of the flame side of the vessel or boiler tube. This can best be done by interposing a thermal resistance between the flame side of the vessel and the water side, such that the flame side may become far hotter than the water side, say a red heat. The writer's researches have shown that when the usual flame impinges on a surface that is artificially maintained at a very much higher temperature than boiling water, say a dull-red heat, the resistance of this film is very greatly reduced; and that the artificially added resistance required to do this is far less than that of the film was, hence there is a great reduction in the total resistance and therefore a great gain in the flow of heat. It is a curious case in which the adding of still more thermal resistance in the path of the flow of heat diminishes the total resistance greatly. In mechanics a spring may in some respects be likened to a resistance to an opposing force, yet the addition of a spring between a violent push and a heavy body helps to overcome that effect of the inertia by which it acts like a high resistance.

These thermal relations are illustrated diagrammatically in Fig. 1. Let the vertical distances represent the thermal resistances in the path of the current of heat from flame to water, and the horizontal distances the temperatures of that side of the vessel which is exposed to the flame; that is, the side on which this film forms. The curve *a* then shows approximately how the resistance of the film diminishes as the temperature of that surface is increased.

To produce this increasing temperature on the flame surface, the added artificial resistance must be increased, as the water side always has the same temperature. The curve *b* represents approximately the respective artificial resistances which must be introduced to produce these increased temperatures. The total resistance, which is what governs the resulting flow of heat, will then be the sum of the ordinates of these two curves, giving a curve approximately like *c*; this curve shows that as more artificial resistance is introduced the total resistance falls, at first very rapidly, then reaches a minimum point and then rises again. The flow of heat will therefore first increase rapidly, reach a maximum, then fall again, showing that there is a point at which it is no longer advantageous to further increase this artificial resistance. The present researches indicate that this point seems to be reached when the temperature of that surface is about midway between that of the flame and that of the water; hence for water boiling this would be about 725 deg. C., which means a dull-red heat. This condition means that the drop of temperature in the artificial resistance is then equal to that in the film.

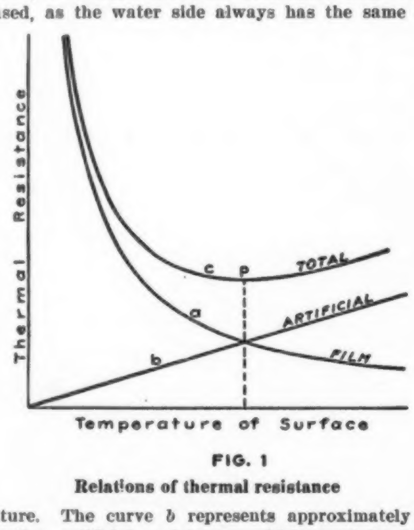


FIG. 1

Relations of thermal resistance

A practical way to introduce this artificial resistance is by means of lugs on the flame side of the surface, which have such a length and diameter that the heat flow through them will maintain their hot ends at about a dull-red heat. In the writer's tests with lugs of the same diameter and increasing lengths, the flow of heat through them at first increased as they were made longer, and then diminished again after a certain length had been exceeded, thus corresponding to the curve *c*. When too long, their ends were at a bright-red heat.

It is an interesting and instructive experiment to solder some small nails or tacks with their heads against the outside of the bottom of a tin cup, then apply a large bunsen flame and notice how quickly and violently the water will boil directly over those nails as compared with the boiling over the rest of the surface of the cup. These lugs may be said to be a means for piercing this high-resisting film, allowing the heat to rush rapidly through these thermal openings.

The same thermal resistance may be produced by a long thick lug or by a shorter but thinner one, provided the ratio of the length to the cross-section is the same; and the quantity of heat flowing through each lug will of course diminish with its cross section. Theoretically, therefore, the best condition would appear to be one lug of the diameter of the bottom surface of the vessel, or in other words, a very thick bottom, or very thick-walled boiler tubes. But it will be found that this thickness (corresponding to the length of the lug) would then have to be several feet, making this form of the resistance absurdly impracticable. The other extreme would be to have innumerable very thin short lugs, close together; this is impracticable on account of the expense, the frailty of such thin lugs, and the fact that when maintained at such a high temperature they gradually burn up. Between these extremes there are mean proportions which give the best results, considering the practical conditions.

EFFECTS OF VARYING SPACING AND SHAPE OF LUGS

Other effects are also involved. By spacing the same size lugs farther apart, the greater freedom of the circulation of the hot gases between them was found to increase the flow of heat through each lug, but as there were then less lugs per square inch of surface, the total heat flow in the vessel as a whole was less. It appears that the film is destroyed, or at least reduced, along the lateral surfaces of these lugs also, as the gases reaching the cooler parts are themselves cooler; hence the lateral surfaces take a more important part than a mere increase of surface. And the lugs may be made slightly conical so that their bases cover practically the whole surface, while their thinner ends are far enough apart to permit the free circulation of the hot gases. When placed radially on the outside of the tubes of a water-tube boiler, they may be cylindrical yet have their cooler ends close together and their hot ends farther apart.

Many comparative tests in which the time was noted for evaporating the same quantity of water over identical flames and in identical open cups, differing only in the size, number and shape of the lugs, showed that there were some best proportions at which the heat flow was greatest, as varying the proportions in either direction gave less good results. These tests also showed very decidedly that the view generally held that a gain by the use of lugs was due to the increase of surface is entirely wrong, which no doubt explains why the frequently suggested addition of lugs and similar surface-increasing devices has not come into general use; the principle was not the correct one. It is of course true that a greater heat-receiving surface is a good feature, but it will amount to little or no gain in the rate unless the thermal resistance of the lugs is properly proportioned. In one test the lugs were made of the same length and total cross section, but had greatly differing surfaces by making one set very flat and the others round; those having the lesser surface actually gave decidedly the better results. The results in many tests were absolutely out of proportion to the surfaces, showing how greatly in error our former views were.

The desired condition is to have such a thermal resistance that when the flow of heat through it has become steady, the hot ends will be maintained at such a high temperature that the film resistance is greatly reduced. With the same resistance the difference of temperature at the two ends will therefore also depend on the flow of heat through it, as a large flow of heat through a low resistance may produce the same difference of temperature between the ends as a small flow through a high resistance; it is quite parallel to the electrical analogy. The proportions, moreover, are different for iron and for copper lugs. It is therefore not only a question of the resistance alone, but also of the resulting flow of heat; the problem

*From *Power*.

of finding the best proportions is therefore not as simple as might at first appear, and the conclusions drawn from experiments must be carefully interpreted or they may mislead.

For instance, a thin coating of enamel or of some asbestos compound might be used as the artificial thermal resistance, but owing to its high specific resistance a very small heat flow through it would suffice to raise the temperature of the outside high enough to break down the film resistance; but as it is a large heat flow that is wanted, the artificial resistance should be made of as good a conductor as possible in order that it may require as large a flow of heat as possible to bring about this film-breaking temperature; the larger this heat flow, the lower need this artificial resistance be.

During the earlier stages of the writer's researches some disinterested parties conducted some carefully made tests in which water was heated by the gas flames of ordinary cooking stoves in open vessels with various kinds of lugs, the amount of gas and its calorific value being determined; their best results were that the same quantity of water could thus be heated about twice as fast with about half the gas; since then the writer has obtained considerably better results.

Referring to Fig. 2, with lugs of the same diameter and of different lengths, as 2, 3 and 4, regularly spaced, the relative flows of heat through a lug were approximately those indicated by the vertical lines above them, that for 1 being the flow through an equal area of the bottom without any lug, therefore representing the normal practice of today. It will be noticed that for the greatest length, 4, the flow again became less, showing that the best length had been exceeded.

In Fig. 3 the lengths of all the lugs are the same, but their diameters are diminished. The vertical lines in this case represent the relative heat flows per unit cross section of the lugs; lug 4 was the same as 3 in Fig. 2. Here again the last one, 6, showed that the best proportions had been passed.

In ordinary boiler practice the normal heat flow is generally given as three pounds evaporated per square foot of heating surface per hour, though this is sometimes exceeded, being said to be as high as 16 in some locomotive boilers, though probably at a considerable sacrifice of thermal efficiency.

The writer's researches were made in open, flat-bottomed tin cups, each having on its bottom a set of regularly spaced equal lugs, the proportions of the lugs being different for each cup, the one without any lugs being taken as the zero of reference. The same quantity of water was evaporated in each, with the same large though quiet bunsen flame, and the time was noted. Reduced to pounds per square foot per hour, some of the many results were as follows:

In the cup without lugs the heat flow corresponded to the evaporation of about 17 lb. per sq. ft. per hour. This and not three pounds should be taken as the basis of the comparison with the lugs.

This rate being allowed for the portion of the bottom which is between the lugs, the rate through the lugs themselves was as high as 467 lb. per sq. ft. per hour, showing how the heat rushes through the thermal openings in this film made by the lugs when they are properly proportioned; this rate is about 27 times that for

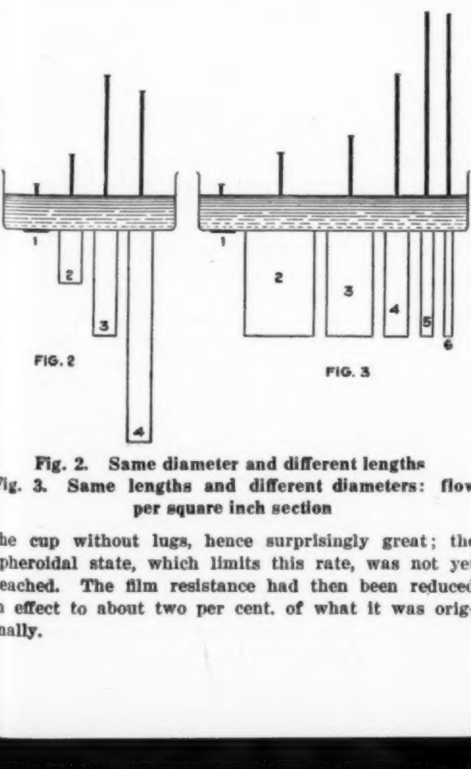


Fig. 2. Same diameter and different lengths
Fig. 3. Same lengths and different diameters: flow per square inch section

the cup without lugs, hence surprisingly great; the spheroidal state, which limits this rate, was not yet reached. The film resistance had then been reduced in effect to about two per cent. of what it was originally.

This shows how very much greater the heat flow through the properly proportioned lugs is as compared with the flow through an equal surface without lugs, and therefore how very greatly the resistance of this film in ordinary practice cuts down the transference of the heat to the water. In that particular test the lugs were spaced rather far apart, in order to find out what might be expected per lug when the hot gases have free circulation around them, and it is therefore of interest only in showing the possibilities and the correctness of the principle. This extremely high rate might perhaps be approached, say half way, in practice in the case of small-diameter boiler tubes in which the cooler ends of the lugs are as close together as possible and the hot ends far apart; but these lugs were rather too slender and numerous for practical purposes, except perhaps for very small boilers or water heaters.

With the same size lugs but spaced much closer together, thus getting less flow of heat per lug but more per square inch of total surface of the whole bottom, the result was about 60 lb. per sq. ft. per hour for the total bottom of the cup, which is still about 3½ times as great as for the cup without lugs.

These lugs were perhaps too slender and numerous for large boilers in regular practice, though perhaps not so for cooking utensils. Making the lugs four times as large in cross section and using less than a third as many, the result was about 55 lb., hence only slightly less and still about 3¼ times that for the ordinary surface.

These results, surprising as they are, could no doubt be still further improved by further researches in this direction. There are other factors which also increase the heat flow, such as ending the lugs in points or edges, making them conical, etc.; moreover, heat seems to flow more readily from copper to iron than the reverse or than from iron to iron, hence copper lugs on iron vessels may give improved results; a moderate blast against the lugs also seems to have a greater effect than against a plane surface.

But even if in boiler practice and for cooking utensils, hot-water heaters, etc., the present rate could be only doubled, it would still mean that the same size of boiler would generate steam twice as fast and probably with even a slight gain in heat efficiency, and for household cooking utensils used with gas stoves it would mean heating twice as fast with half the gas. The bottom of the cooking utensils can be cast with the lugs, and in steam boilers the lugs can be electrically welded to the tubes, hence neither involve anything impracticable or costly.

Tunnels vs. Bridges

"Why do they build tunnels under the river when they could have bridges instead?"

It is not only the middle western visitor to New York who asks that question. New Yorkers are not lacking who express decided distaste for journeying under the river when they feel bridges would be much better.

Fear for their personal safety "if anything should happen," and the apparently unnecessary diving deep underground when blue sky and open view are much more pleasant, are probably the chief factors in the resentment thus expressed—together with one other thing: that the tunnels are as yet new, the whole tunnel history of the city virtually having been made in twenty years. So the idea of the rapid transit tunnels has not yet found a place in the mind comparable to that occupied by the bridge; and the new and strange has ever the elements of uncertainty and insecurity.

Why, then, does a rapid transit corporation prefer and construct the East River tubes instead of spanning the river with the great arch of a bridge as the city has done? The answer is not far to seek, and comes in many forms.

The initial cost is one great item. The Brooklyn bridge, opened to traffic thirty-five years ago, cost for construction alone approximately eighteen millions—a sum which would be vastly increased if it were to be duplicated today. Land necessary for approaches cost nearly half as much more, and that cost, too, would be multiplied manifold today. The actual cost of construction for one of the river tubes is something more than three millions.

The second big item in expense is maintenance, which in a tunnel is practically negligible once the building is finished. With a great East River bridge, the maintenance account runs into six figures annually.

Of course, it must be understood that these figures are on the face of them unfair to the bridges in that they do not afford a direct comparison of services rendered. It must be remembered that the bridges carry four double-tracked areas, divided between surface and rapid transit, besides accommodations for the streams of vehicles, both horse and motor-driven, and the hurrying throngs of pedestrians for whom a rapid transit tube makes no provision. The one is a manifold answer to a city's problem; the other is the simplest, most efficient and expedient of a rapid transit corporation.

That very difference brings about a divergence of interest which, when the matter of operation comes to be considered, is readily apparent. The bridge, higher in the center than at the ends, causes a train to use power from either end to the center of the arch, and then dissipate power in the brake-shoes on the descent from the arch to the other end. A tunnel is built with the exact opposite result; the train coasts down from either end, gathering energy, which helps to carry it up the ascent with the least possible expenditure of energy. The railway empire builder, Hill, attributed his success largely to the original planning of his roads to obtain the lowest possible operating cost, even though a greater initial outlay were necessary. Hence, for a rapid transit corporation, the double advantage in this field.

Aside from these factors, the relative merits of bridges and tunnels for rapid transit service take on an air of open field discussion. The questions of permanence and endurance, and of adaptability to changing conditions of traffic are not without warm advocates on both sides. The tunnel engineer points with pride to the tube he has constructed, with a concrete lining, through solid rock; or to the iron-cased, concrete-lined passageway through softer material, and believes that hundreds of years will find it still available. In fact, the warmer advocates of tunnel engineering believe that in traffic tunnels lie the solution of New York's great and growing traffic congestion problem. Larger tunnels, with provision for vehicular traffic and pedestrians, they believe, will come into being wherever the traffic problem warrants, probably following largely the present ferry routes. Some of the more enthusiastic of these tunnel engineers even predict that no other great bridge will ever be built as have the four East River bridges, for solving the traffic problem of Greater New York. And the adherents to the bridge solution point to the projected tri-borough bridge plans, which would connect St. Ann's Avenue, Bronx, with Potter Avenue, Queens, across Randall's and Wards' islands, with a Manhattan connection on Randall's Island from 125th Street; a bridge which would make possible a direct Bronx-Brooklyn rapid transit service.

In the other objection raised by the travelling public—that of safety—there are again two sides to the question. The tunnel engineer exhibits cast iron casing more than an inch thick, with thirteen inches of solid concrete within as evidence of a strength endurance and permanence which laugh to scorn the timorous who hesitate about venturing below the river bed in rushing trains through the dim tubes, and breathe a sigh of relief when they once more stand in the open air. Perhaps the first journeyers by train and tram felt that way quite as strongly. Those who cling to their belief in the all around superiority of bridges maintain that this psychological factor is really very important; that the sense of being in the open, able to see where one is going, discounts a great deal. Certainly a derailed train in a tube has nowhere to drop and can only come to a stop against the iron posts, and a tube has not to meet such wear and tear of the elements as a bridge. With equal certainty the word of the doubter persists: "But you can't see where you're going down there, and if anything should happen—" "But nothing could happen," is the answer of the tube experts.

At any rate, the part tunnels will play in the future for the relief of traffic as well as transit congestion will be one of the things New Yorkers will watch with profound and growing interest. At present the East River holds, besides the original Interborough tubes, the Old Slip-Clark Street tunnel, which will connect the present subway in Brooklyn with the new Seventh Avenue subway in Manhattan; the Montague Street-Whitehall Street tubes for the Broadway-Fourth Avenue subway; the Fourteenth Street tunnel, still a long way from completion; the double Pennsylvania tubes at Thirty-fourth Street; the Belmont tunnel for the present Queensborough subway; the Sixtieth Street tunnel through Blackwell's Island, which will connect the Broadway-Fourth Avenue subway with Queens direct; and the gas and water tunnels.

So the honey-combing of the greater city goes on. Transit arteries below its rivers; traffic arteries across its rivers; what does the next half century hold?—*B. R. T. Monthly.*

Long Range Temperature Forecasts

A Method of Comparing Weather Records with a View to Predicting Future Conditions

By Frank S. Wood

In 1916, the writer, while plotting a chart of the annual mean temperature for Boston, Massachusetts, happened to use a ruled paper divided into groups of twelve lines. As a result of that occurrence, when the chart had been finished which included the records from 1871 to that date, it was observed that a high temperature recurred at the end of every group of twelve lines and that this was the highest temperature during that period. This was the proof of temperature cycles. Taking this evidence as conclusive, further study made it obvious that in this movement of temperature was concealed the natural law which governed temperature changes. If the law could be found, long range forecasting of temperature would not only be possible but practicable. From that time on, an intimate analysis of the records was made with the object of perfecting a method of forecasting which should be sound and mathematical and wholly free from the personal equation as well as the element of guesswork. The writer believes that such a system is now demonstrated in which the forecasts of temperature for a long time ahead, will now be verified in over 80 per cent. of cases, while further study and research will largely increase this percentage of verifications. The reasons for such belief are that forecasts made a year ahead, by months, have already been verified accurately, as above. As the value of an invention consists not in the idea but in its reduction to practise and the results attained thereby, so the value of this method of forecasting is proved by the accuracy with which forecasts are verified. The proof of the pudding is in the eating. However, much or little the reader may agree with the reasoning of the writer, the obtaining of unheard of results and having forecasts verified, month after month, is or ought to be sufficient evidence to convince the most skeptical that long range forecasts are being made. It is not within the range of possibilities for chance to enable a mathematical calculation to be correct in a long series of sequential forecasts. If the figures were based upon false premises, no three forecasts, in sequence, could by any possibility be verified. When twelve consecutive months have their forecasts verified, he who would refer the results to chance can only be classified among those who close their eyes and deny that there is light.

At first, it only seemed within the range of probability to me that it would be likely to forecast years of extreme heat and cold which might be verified but an intimate analysis of the chart of temperature largely broadened the range of practical probabilities. For example, once it was determined that there was an orderly march of temperature from a high point to a low point, covering a period of years, many of which were known a long time in advance, it became equally clear that a more intimate study, of the monthly temperatures of specific years, ought to determine what months in those years were likely to be prominent in influencing temperature in a given direction. Thereafter, it was carefully worked out by months, on the same basic principle which had given the verification by years, and it was found equally true with monthly temperature, namely, that months of extreme heat or cold were correctly indicated in more than 80 per cent. of forecasts. This ought to be good enough, as a start, for any new method. At any rate, these are the results thus far achieved. The forecasts make good. Throughout this account, the data used and the section covered by it is that of the Boston forecast district, unless otherwise specified, therefore in the event that a statement is made which is not clear in its meaning, it always refers to Boston and vicinity.

It may be of interest to describe the steps by which the method of making long range forecasts of temperature was worked out. As already noted, the first step was the charting of the temperature by yearly means, which figures are obtained by averaging the monthly means of each year; the latter are in turn obtained by averaging the daily mean temperatures of each month. It was first observed that the chart of annual mean temperature showed that the temperature had a constant variation; that from a highest point in a group of years there was an orderly downward movement to a lowest point and a return to another highest point, and so on. Then it was noted that each group of years extending from one high point to the next was very similar in general character to the other groups. Then it was obvious that there was a temperature period or cycle, during which the temperature, starting at a high point, declined to a lowest point in that period and then rose to another high point within 12 years. As there are twelve months in the year so there are 12 years in the temperature period. At least, that is the nearest that it can be measured. It has been noted to occur in 9 years and again in 13 years; the average for the last 117 years beginning in 1801 was 11.3 years. If, under certain circumstances, a record exceeds 11 years, it requires 12 calendar years to complete the period. The final year of the rise which completes the cycle, serves, when completed, as the first year of the next cycle. The next feature of the temperature chart to be comprehended was that the orderly march of temperature through each cycle was by means of constantly varying reciprocating movements. From the highest point in each cycle, the temperature takes a step down, followed by a step up, another step down and another step up, and so on. A step may cover one year or several years but the steps all lead to the same end, namely, from the highest point to the lowest point and back again to a new highest, with 12 years as the measuring stick of the average cycle. The temperature data was then tabulated by groups numbered from 1 to 11 respectively, the first year of each cycle being averaged with the first year of all the other cycles, which gave a normal temperature for that first or hottest year of the cycle, and so on with all the other years. As the result of this composite column of figures, we have a table representing the normal temperature of each year in the cycle. This normal temperature varies every year; starting at the highest, it reaches the lowest point the fifth year, as a rule, reacting therefrom the two succeeding years and declining again to a low point the eighth year of the cycle. The temperature may be lower the eighth year than the fifth or it may not be quite so low, but both years may be looked forward to with confidence as being relatively cold years. From the eighth year, there is a rapid rise to a new high point beginning the next cycle.

This description embodies the skeleton or frame work of a new science in temperature forecasting. The flesh and blood of the system is supplied by what follows. The brain and nerves will only be annexed by further study and research upon these lines and for many years to come.

Several new facts are established by the foregoing, foremost among which is the cycle of temperature. While it is of great importance to be able to accurately forecast years of extreme heat and cold, it seems to me to be of no less importance to establish a standard of measurement for temperature so as to be able to demonstrate whether or not any perceptible changes are taking place and, if so, what those changes may consist of. Moreover, the fixing of a normal temperature for each year of the cycle is a great step in advance. As used in describing temperature by the Weather Bureaus, the normal is the average of all the years of record. Instead of being the expected temperature, it is that which occurs the most seldom. On the contrary, the above described method of obtaining a table of normal temperatures results in obtaining a set of figures within the true meaning of the word, and any departure from the normal in a given year of the cycle is, with rare exceptions, but a fraction of a degree. For example, the expected or normal temperature for the year 1917, in Boston, by the table before mentioned was 48.2 degrees; the actual record was 47.9 degrees; while the so-called Weather Bureau normal or mean was 49.4 degrees. Again, using the 12-year measuring stick, it is found that the temperature of Boston has been slowly but surely increasing. By the law of averages, Boston annual mean temperature has increased 1½ degrees in 36 years or at the rate of ½ degree for each 12 years. The monthly changes are even more striking and will be elsewhere described. Below is given the table of annual mean temperature, for Boston, arranged in cycles and in regular years of each cycle, and the last column of which is the normal, or temperature to be expected, in each year of the cycle.

Boston Temperature Cycles

					Year	Normal
	1878-50.2°	1889-50.7°	1900-50.8°	1913-52.3°	1st	51.0°
	1879-48.4	1890-49.1	1901-49.0	1914-49.7	2d	49.1
	1880-50.3	1891-50.4	1902-49.6	1915-51.2	3d	50.4
	1881-49.6	1892-49.4	1903-49.5	1916-49.7	4th	49.5
1871-48.9°	1882-48.8	1893-47.9	1904-47.1	1917-47.9	5th	48.1
1872-48.8	1883-47.9	1894-50.3	1905-49.1		6th	49.0
1873-48.2	1884-49.0	1895-49.8	1906-50.0		7th	49.2
18.4-48.6	1885-47.6	1896-49.2	1907-48.7		8th	48.5
1875-46.6	1886-48.4	1897-49.9	1908-51.2		9th	49.0
1876-47.9	1887-48.5	1898-50.8	1909-50.5		10th	49.4
1877-50.1	1888-47.3	1899-50.2	1910-50.8		11th	49.6
			1911-50.9			
			1912-50.5			
Average	48.7°	49.9°	49.8°			

The figures of this table are the official data published by the Boston Weather Bureau. When plotted upon a chart their relations to one another and to the cycles which they represent become obvious. See Chart. While the normal figure for the fifth year is now 48.1 degrees, in the paragraph preceding the table, I gave the figure upon which the forecast was based before the temperature of 1917 was known.

It should be carefully noted that the first year temperature for each cycle has always exceeded the previous highest mark. This is another bit of evidence that the temperature of Boston is increasing. There is no getting away from that fact which can neither be evaded nor avoided.

Applying the forecast to the last column of figures, it will be seen that the normal temperature to be expected for 1918 is 49 degrees or about a degree higher than the record of 1917. Special attention is called to the fact that in every cycle, thus far, the changes from the first to the fifth year of the cycle have been regular. From the high point there has been, in each instance, a step down, a step up, and another step down, the latter occupying two years. From this year on, there is greater irregularity in variations.

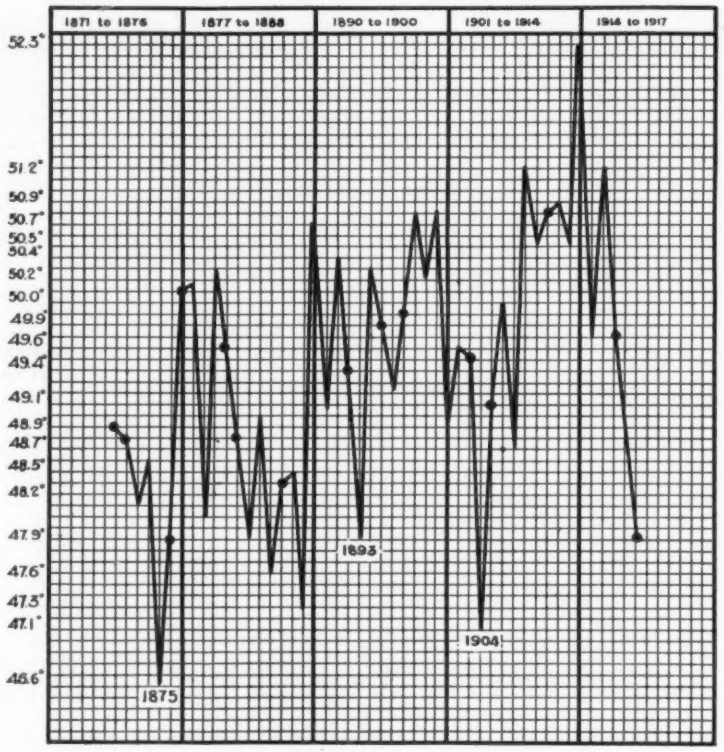
Without going farther, the natural law governing temperature changes was revealed by the chart and the table above given. In a nutshell, it may be briefly stated as follows:

Extremes of heat occur the first year of the cycle, followed by reciprocating changes until the fifth year when extremes of cold occur. After reacting from this extreme another extreme of cold occurs the eighth year followed by either a reciprocating rise or a sharp rise to the beginning of the next cycle.

This law governs temperature changes in a zone several hundred miles wide and in which Boston appears to be well within its boundaries. It does not govern sections far removed both north and south of this zone but the changes in these far-removed zones are governed by modifications of this law similar to the law of climate and modifications thereof due to latitude and altitude. In other words, the forecasting of temperature by long range methods is not simple. The establishment of a fundamental principle for the Boston zone of efficiency does not establish that principle or any other principle for other zones. Each zone has a character and law of its own, upon the successful discovery of which depends the application of the principle to forecasts and verifications thereof.

Boston is one of the few if not the only Weather Bureau station in its zone that has records which are reliable to indicate changes of temperature covering substantial periods of time. Other stations in other cities almost everywhere in the United States have experienced one removal after another where the thermometer exposures have been so changed as to completely veil any real change of temperature. It has been actually proved by demonstration, in Chicago, that a difference of 1.7 degrees resulted in a single removal. The New York station is now 414 feet above the street and has resulted in lowering the temperature

about 1 degree. Unless temperature is recorded under similar conditions and exposures, the records are rendered worthless for purposes of truthful comparison.



Annual mean temperature at Boston, Mass.

Therefore, when it is understood that the change of temperature in Boston is but $1\frac{1}{2}$ degrees in 36 years, it may readily be perceived how little value the records of New York and Chicago possess when a removal in one year offsets all the real variation in true temperature. Until the Government adopts a policy of establishing Weather Bureau stations nearer the ground and where the thermometer exposures may be constantly the same, its records are absolutely worthless to posterity if any comparative deductions are desired. The temperature data of New York, Philadelphia, Washington, Buffalo, Detroit, Chicago, St. Louis, and many other cities, have been examined, charted, tabulated, and compared with Boston and with one another. Corrections for removals have been impossible, but enough irregularities were established and accounted for to support the facts established at the Boston station. I make no hesitation in stating that they corroborate, in general, the determinations derived from the Boston data. The charts vary in details but indicate much that cannot be demonstrated owing to the unfortunate temperature changes due solely to removals to higher buildings and lower temperatures than before, and vice versa. The Boston thermometers have had the same exposure since the Weather Bureau was established and only one removal to its present quarters in 1885, therefore its records have an unusual value which are excelled by no other station in the United States. But this was unknown to the writer until after years of study and research.

Having pointed out the factors which express the law of long range forecasting of temperature changes, many of my readers have no doubt already formed the conclusion that the truth is out and anybody can do his own forecasting. But we are dealing with yearly temperature which takes 365 days to record one entry and in which there is little satisfaction in being able to make a successful forecast. Let us step out of the year into the month and see what happens in our new surroundings.

We now know that there is a certain zone in which relatively high temperatures may be expected at the end and beginnings of each cycle and that relatively low temperatures may be expected between the high points. One might anticipate that these annual changes would proportionately influence each month of any specified year of the cycle. If so, that anticipation would be in error. Such simplicity, if it obtained, would long ago have been observed. The character of temperature is complex, not simple, but has a character based upon natural law just as well as the 12 different motions of the Earth. The backbone of that character is the vertebra comprising the normal temperature of each cycle year. As each year of the cycle differs from every other year, so each month of each year differs from every other month of that year and every other year. The complexity now becomes so apparent that the wonder is that it may ever be mastered. However, the complications are more apparent than real as will presently be seen. Applying the keynote of temperature change to each month of the year, as in the table of annual temperature, and another table is provided for each month which shows wherein it is in tune with the annual change and wherein lies the discord. In this table we have the normal temperature of that month during each year of the cycle. The figure which represents the expected temperature of each year of that month is the approximate temperature to be recorded in the future. There are exceptions, occasionally, which are reasonably accounted for and with more accurate study and observation may, in time, be successfully anticipated. The method is by no means perfected but it is even now as successful as any

Boston Tabulation for December

					Year	Normal
	1878–30°	1889–38°	1900–33°	1913–38°	1st	34.8°
	1879–32	1890–26	1901–32	1914–30	2d	30.0
	1880–27	1891–40	1902–28	1915–34	3d	32.3
	1881–39	1892–30	1903–29	1916–33	4th	32.7
1871–28°	1882–30	1893–30	1904–26	1917–24	5th	27.6
1872–23	1883–29	1894–32	1905–35		6th	29.7
1873–32	1884–34	1895–36	1906–29		7th	32.7
1874–31	1885–33	1896–30	1907–37		8th	32.7
1875–30	1886–28	1897–34	1908–33		9th	31.2
1876–22	1887–32	1898–32	1909–31		10th	29.2
1877–36	1888–35	1899–36	1910–28		11th	33.7
			1911–39			
			1912–38			

method of weather forecasting, which promises much by future development. In forecasting the temperature of any month, it is of no special value to predict the actual temperature to a degree. The information desirable and valuable is to know if extreme cold is to be experienced, if extreme heat will take place, if average or seasonable temperature will occur, and finally if a temperature between the average and either extreme is likely to appear. This establishes five different degrees of temperature which are of practical benefit to the public, which may be classified as follows: Severe cold (for that month); moderately cold, seasonable temperature, moderately warm, and extreme heat (for that month). Provided that any degree of temperature is forecast which brings it within the above classification, there can be no doubt that the forecast may be checked as verified. To illustrate, a table of monthly mean temperatures for December is submitted:

This month corresponds, in its extremes, with the annual table. The first year averages the mildest and the fifth year as the coldest. In the column of normal temperatures, it is seen that 34.8 degrees is the highest average but the actual highest was 39 in 1911, consequently any temperature as high or higher than 34.8 degrees would be extreme heat (for that month). On the other hand, the lowest average (normal) is 27.6 degrees but the actual lowest was 22 degrees in 1876, therefore any temperature as low or lower than 27.6 degrees would be extreme cold (for that month). It is obvious that a temperature from 30 to 31 degrees would be seasonable while any temperature between these figures and either extreme would be proportionately and moderately warm or cold, as the case might be. As the normal figures change with each cycle, it is only practicable to compare those in the table with the actual figures from 1913 to 1917 inclusive. Note the result:

The first year, 1913, extreme heat was forecast and the normal figure was 33.6 degrees at that time. The actual record verified the forecast, being 5 degrees above the normal heat extreme. The second year, seasonable temperature was forecast and was verified by the record; the third year, moderately warm was forecast and verified; the fourth year, ditto; the fifth year, extreme cold was forecast and verified, the actual record being over 4 degrees colder than the normal up to that year.

In all probability, we have seen the coldest Deecember, the past winter, which will be had until 1928, when another December of severe cold is forecast. Here is a concrete example of the long range to which temperature may be forecast.

In contrast with the large percentage of verifications in December forecasts, the month of January, at present, has the most exceptions. The normal figures, for January, forecast extremely cold weather for both the fifth and sixth years. The forecast for the fifth year was in error, the actual record being slightly higher than seasonable; but the sixth year fell into line again, being 4 degrees below the normal for that cold year and only 1 degree above the lowest ever recorded.

CHARACTERISTICS OF TEMPERATURE CYCLES BY YEARS AND MONTHS

The following table furnishes the index numbers for Boston, Mass., for each month in each year of the cycle:

The figures in bold face type are the months indicating extreme heat and the italic figures are the months indicating extreme cold. These refer to the relative heat and cold of each special month with regard to the records for that month. Each figure is the mean for that month and year of all the cycles thus far recorded from 1871 to March, 1918.

Index numbers which correspond to seasonable temperatures need no correction. Those indicating extremes require correction to turn the index numbers into actual degrees expected, as will be elsewhere described.

The table below given is typical of the Zone No. 1 stations which extends broadly from Montreal, Canada, to Washington, D. C., Boston being well within both northern and southern limits. It is very closely related to the North and Middle Atlantic States, classified as Area No. 1, in 1918, by the U. S. Weather Bureau. While many of its characteristics are found as far west as St. Paul, Minn., important changes are found at Buffalo, N. Y., and still more in Chicago, Ill. Along the northern boundary of this zone, certain months in years which will be specified appear to be dominated by the influence of the next zone north of it; while along the southern border a similar influence is noted as modifying certain months to harmonize with the different character of the zone just south of Zone No. 1.

Year	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Annual
1st	**33.3**	28.5	**38.5**	**48.3**	56.5	67.8	72.8	69.8	63.5	54.3	44.3	**34.8**	**50.1**
2d	38.3	26.5	35.5	44.3	57.8	66.0	71.0	69.8	63.7	**55.0**	40.3	30.0	49.1
3d	31.5	**31.5**	36.5	**48.3**	**58.8**	65.5	*69.8*	69.5	**65.5**	53.5	42.5	32.3	50.4
4th	27.5	28.2	36.2	46.5	57.5	*63.8*	71.7	69.5	64.7	54.2	42.0	32.7	49.5
5th	*25.0*	27.0	36.8	44.6	55.4	65.6	71.8	**71.2**	*61.0*	53.4	*39.0*	*27.6*	*48.1*
6th	25.4	26.8	34.0	46.5	57.7	**68.2**	**73.7**	69.5	62.7	52.0	41.7	29.7	49.0
7th	28.5	28.0	33.7	45.5	57.5	66.7	70.2	70.7	65.2	52.2	40.2	32.7	49.2
8th	27.2	*24.7*	*33.0*	*44.0*	*55.0*	66.0	72.2	*69.2*	62.5	*51.2*	43.7	32.7	48.5
9th	26.2	26.7	35.0	47.0	58.0	65.5	72.2	69.5	63.2	52.7	40.7	31.2	49.0
10th	28.7	30.5	36.2	45.0	56.1	67.2	73.2	70.0	62.5	51.7	42.7	29.2	49.4
11th	26.5	29.7	35.7	46.7	56.0	67.2	72.0	70.0	62.5	52.5	42.7	33.7	49.6
Seasonable	28 to 30	27 to 28	35 to 37	46 to 47	57 to 58	66	71	69	63 to 64	53 to 54	42 to 43	32 to 33	

FIRST CYCLE YEAR

Referring to the table, it will be seen that the index number shows that the annual mean temperature is noted for extreme heat in the first year of the cycle. While the annual index number indicates the general character of temperature for the year, the monthly index numbers show what months are most influential in producing this highest annual mean temperature. It will be noted that the months of January, March, April, June, November and December are the only ones giving large excesses of heat. Of the others, February and September are seasonable or average; May and August are moderately cool; and July and October are moderately warm. These characteristics appear to be common to all places within Zone No. 1.

SECOND CYCLE YEAR

The second year of the cycle shows a substantial drop in annual temperature, especially noteworthy in those months which showed the greatest excess of heat in the first year. This is one of the few years chiefly noted for its moderate monthly temperatures. The exceptional months are April which is usually very cool and October which shows extreme warmth for that month. This characteristic, however, is not common to all places within Zone No. 1, as this is one of the years when substantial modifications occur. Thus, in the north, Montreal for example, extreme heat occurs in May as well as October and extreme cold in November. In Washington, a cool September occurs. At places between Boston and these other two cities, there are mixed conditions partaking of the character of both.

THIRD CYCLE YEAR

In the third cycle year, an upswing in annual temperature occurs, in which the months of February, April, May and September are conspicuous examples; while July and August are very cool. Other months are either moderately warm or seasonable. There is less excess of heat along the southern border of Zone No. 1 and in Washington. On the other hand, Montreal, which had its warmest May a year earlier, now has only moderate heat in that month but has a greater excess in March. The other months are in harmony with the general characteristics of the zone.

FOURTH CYCLE YEAR

This year brings another drop in temperature similar to that of the second year, with a warm May and a cold June as conspicuous extremes. The only other noteworthy variation is a warm August along the southern border of the zone.

FIFTH CYCLE YEAR

The fifth year brings a continuation of the downswing which, as a rule, results in the coldest annual temperature of any year in the cycle. It brings extreme cold for January, May, September, November and December and moderate cold for April. Only one month, August, brings excess of heat, the other months being seasonable.

SIXTH CYCLE YEAR

In this year, a substantial rise of temperature occurs from the low point of the fifth year. Only one month, January, indicates extreme cold; March, August and October indicate moderate cold; May brings moderate heat while June and July show extreme heat, and the other months are seasonable in the central section of the zone. This is another year which brings modifications along its borders. In the north, the extreme heat indicated for the central section is modified to seasonable conditions; while in the southern part of the zone extreme heat in May is added to the other summer heat.

SEVENTH CYCLE YEAR

With the exception of a cool July and a hot September, the seventh year of the cycle is devoid of extremes in the central part of Zone No. 1. But at both northern and southern extremities the month of June brings extreme heat. There is a gradual warming up tendency in most months this year throughout this zone, excepting extreme cold in the southern part during March.

EIGHTH CYCLE YEAR

This year brings the final reaction to low temperature, for the cycle, it being a toss-up whether this year or the fifth year will prove to have the lowest annual temperature. It is a remarkable fact, however, that none of the months registering extreme cold in the fifth year are found in the extreme column this year, possibly excepting May. Now, we find February, March, April, August and October bringing an excess of cold, with May from moderate to extreme cold. In the south portion of the zone a moderately warm January, July and December occur.

NINTH CYCLE YEAR

The ninth year is like the sixth year in annual temperature but the central section of the zone has no months of extremes. The winter months are warmer and the summer cooler with seasonable temperature throughout, excepting a moderately cold January. In the southern part of the zone, however, exceptions occur in which extreme cold is indicated in February and August; while in the northern part June brings extremely cool weather.

TENTH CYCLE YEAR

The tenth year is substantially warmer than the preceding but there are few extremes. February and July show excesses of heat; while in the south portion, only, December shows an excess of cold.

ELEVENTH CYCLE YEAR

A further warming up takes place this year which is frequently the last year of the cycle, although there are exceptions. The north and central portions are usually without any extremes. An occasional exception, however, occurs which brings excessive heat to New York City in June and August and in August alone in the south portion.

The foregoing table and description establishes a general rule for long-range forecasts which are verified in about 75 per cent. and to which there are exceptions in about 25 per cent. of indications. One illustration of an exception will furnish an example of the kind of work which is still required in order to increase the percentage of verifications. In January of the fifth year, extreme cold is indicated for the No. 1 Zone. It was verified in the northern part of the zone but the cold did not extend as far south as Boston. The sixth year, moderate cold was indicated throughout the zone; but extreme cold was the record. The conclusion is that extreme cold is to be expected in January the fifth year of the cycle but it may not come until the sixth year at central and southern points in the zone. There is good ground for predicting the exception, based upon what part of the year the turn of the cycle takes place. If it happens in the first part of the year, extreme cold comes in January of the fifth year; on the other hand, if the turn of the cycle comes in the last part of the year, then extreme cold comes in January of the sixth year.

SEASONABLE TEMPERATURE

It may be well to define what is meant by seasonable temperature. The Weather Bureau calls the average temperature seasonable; but in many places and in many months, the average is constantly changing. For instance, in Boston, the average temperature of the month of January is 28. The average for the first twelve years of the record was 26.6. The average for the last 12 years was 29.8, and within the last 14 years it has been as high as 31; while it has not been below 28 during the last 20 years. It is clear, then, that the range of seasonable temperature is from 28 to 30. For other months than January, seasonable temperatures have been arrived at in the same way.

CORRECTIONS

Since the last cycle began in 1913, which was the first year, it is simple to trace the temperature through any year. One thing which cannot yet be anticipated is the number of years in the cycle. The preceding cycle contained 13 years and that was the first cycle within the records of the Weather Bureau which contained more than 11 years. Whether the present cycle will resume the average of 11 years or whether it will be less, remains to be seen. If it goes back to the 11-year period, 1924 will be the first year of the next cycle, but this may not be certainly known before the next year of minimum sunspots is determined.

It has already been noted that this tabulation shows, in rare instances, that one place will have an extremely cold month a certain year of the cycle while another place will have extreme cold the same month in the year following. Similar instances have been noted with regard to months of extreme heat. Should further tabulations increase the number of instances of this character, it is readily seen that this feature alone will result in anticipating exceptions to the rule to a large degree of efficiency. But this is not the only source of information, in this respect, although it is a very important one which should be taken advantage of at the earliest possible moment.

One peculiar fact in regard to the January index numbers is that notwithstanding the wide range of seasonable temperature, from 28 to 30, only 3 months in the cycle come within this range.

Another fact derived from the study of February variations is that the temperature of that month has grown colder while other winter, spring and fall months have grown warmer. February is now the coldest winter month when averaged in 12-year periods, through the central and southern portions of Zone No. 1, and extending to places far south.

August is another month showing a decline in temperature.

In conclusion, the following forecast is given for the balance of this year and the first half of the year 1919. The figures given are for Boston but the wording of the forecast for each month applies equally to all places within the Zone No. 1 subject to border modifications already described.

1918 FORECAST:

May, moderately warm—60; June, extreme heat (for that month)—70; July, extreme heat—74 to 77; August, unusually cool—67; September, seasonable—62; October, moderately cool—50; November, seasonable—42; December, moderately cold—27.

1919 FORECAST:

January, moderately cold—27; February, seasonable—28; March, moderately cold—29; April, moderately cool—44; May, seasonable—58; June, seasonable—67.

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